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Editor: TREVOR I. WILLIAMS, M.A., B.Sc., D.Phil., F.R.I.C.
Editorial Assistant (Foreign Editions): G. N. J. BECK, B.Sc., A.M.I.E.E.

Imperial Chemical Industries Limited, Millbank, London, S.W.1
Communications to the Editor should be addressed to North Block, Thames House, Millbank, London, S.W.1

Internationalism in science

The International Geophysical Year that began in July 1957, after more than five years preparation, is the greatest single programme of scientific co-operation ever attempted. Its present success is a welcome reminder of what far-reaching scientific results can be achieved by friendly co-operation between nations, but nevertheless it cannot fail to provoke thoughts of fields in which this spirit of harmony is noticeably lacking.

Scientists of the present generation were educated in the belief, which still persists, that science knows no frontiers, and that if its material rewards are modest, at least it is above the international strife which bitterly divides men in so many other activities. History could provide some comforting illustrations to support this belief. Rumford was honourably received in Paris even when France and Britain were in the throes of bitter war; Benjamin Franklin, during the War of American Independence, gave special instructions that Captain Cook was not to be molested by American cruisers while on his voyages of scientific exploration. It is true that in very many fields national frontiers are still unimportant: collaboration is limited only by the initiative and resources of individuals, and there are no restrictions upon publication. The areas of restriction may be relatively small, having regard to science as a whole, but they are of exceptional significance and represent very great expenditure in both money and manpower.

We have here a strange paradox. On the one hand, there is general agreement that many of the great material problems of the world will not be solved without the widespread application of new scientific and technological discoveries. On the other, we see a vast and unnecessary duplication of effort, especially in the fields that promise to be the most rewarding, at a time when even the most far-sighted nations have a great shortage of scientists and technologists. The political reasons for this paradox are familiar enough. As long as there is deep distrust between nations and groups of nations, none, very obviously, will risk sharing with others secrets upon which survival might depend. Even between firm allies the free exchange of knowledge is manifestly difficult, for the tide of war might well cut off one partner from another, and in consequence every major nation feels an urge to be as self-sufficient as possible.

Distrust between nations is as old as civilization, and it would be naive indeed to suppose that the intensely difficult political problems of today will wilt away if we remove artificial barriers to the onward march of science. Nevertheless, science is undeniably the biggest single factor determining the future of mankind, and it is of the first importance that the fullest possible use be made of the possibilities for material advancement that it offers. This clearly cannot be done while progress in some of the most potentially fruitful fields is artificially restricted, as it is at present.

The development of thermonuclear power is a topical and striking example. Here we have a project that seems destined ultimately to provide the world with cheaper and more abundant power than could have been dreamed of even twenty years ago. The possible benefits to mankind as a whole are incalculable, and if ever there was a field in which one would wish science to transcend national frontiers this is surely it. Yet it has not done so. The publication of scientific results of the first importance has been delayed for political reasons, and co-operation between the various national research centres has been at best belated and incomplete, at worst non-existent. There are always those who can advance good reasons why an obviously unsatisfactory state of affairs is best left unaltered, and in the absence of counter-argument they will carry the uninformed with them, but the present situation is one that all scientists must deplore and seek to change.

In present circumstances the scientist has a particular interest and duty in the achievement of international relations cordial enough for all his resources to be deployed for peaceful ends. Like every other person, he has an acute personal interest in surviving in peace rather than perishing in war. But beyond this, his special knowledge encourages him to see the dawning of a golden age, such as the world has never known, in which the fruits of science, wisely applied, can be increasingly used for the benefit of mankind. Science alone can solve few problems, but, given opportunity and the co-operation of others upon whom our complex civilization depends, it can certainly remove many of the material causes of human unhappiness. Science as a dominant force in civilization is very new—which in itself is doubtless an important reason why its achievements so far outstrip general recognition of both its possibilities and its dangers—but its beginnings can be traced back for many centuries. A world in which man's

mastery of nature is beneficently applied seems almost as far away now as when it was adumbrated by such far-sighted men as Francis Bacon in the seventeenth century. It is tantalizing that a prize so rich and so long sought cannot be enjoyed to the full, and may even be lost again for generations, because of bitter human differences.

Many scientists see clearly enough how much might in favourable circumstances be achieved by the full exploitation of our present knowledge, let alone of the knowledge that may reasonably be expected to accrue in the next few decades. But are these possibilities clear to those who shape national policies and, above all, to those who elect to office those who shape such policies? Unhappily, they are not. A great part of the world still fears science and sees in it not a benefactor but a destroyer: this is scarcely surprising when one of the greatest scientific discoveries of all time has been primarily exploited as a threat between nations.

But if politicians have been, and still are, slow to see the full implications of new discoveries, scientists themselves are not blameless, for at a time when they should have given a positive lead they have too often been inactive. Though organizers of the greatest single material force in the world today they have as a group done little to inspire the public with the truly great possibilities arising from it. It is true that the popular exposition of science has improved enormously during the past decade, both in quality and in quantity, but the initiative has in the main come from outside science and not from within, and still does so. Scientific men are usually ready to give advice when it is called for in a good cause, but how few have exerted themselves to make scientific ideas known where they would never have been sought and to propagate their own faith in science. Ability to spread new ideas among those who see no particular need for change is unfortunately not necessarily acquired at the same time as scientific education and experience. It demands personal qualities that are doubtless as rare among scientists as they are among the population at large, but they certainly exist, and should be much more widely exercised than they are. The main explanation is doubtless that the absorbing interest of science tends to make those devoted to it reluctant to leave it for other fields.

Industry has long realized the importance of good public relations, and its policy of actively promoting these has unquestionably been a powerful factor in its recent great development. In science the position is altogether different; any sort of public relations organization is looked upon by some scientists as not only unimportant but even as in some way unbecoming. This attitude is gradually changing, more rapidly in some countries than in others, but the few agencies that now exist for the primary purpose of disseminating scientific information to the public too often labour not merely under financial difficulties but even without the degree of support they are entitled to expect from scientists themselves.

Replacement of the fear of science by realization that if properly used it offers tremendous benefits for mankind is of the utmost importance, not least to science itself. It is now not merely possible but important to go further than promises, and to provide tangible evidence of what can be done if scientists from all nations unite upon some great practical project, as they are at present doing in the I.G.Y. One such project has lately been proposed: it is for the eradication of malaria. This is a disease believed to affect some 700 million people and to cause perhaps 10 million deaths annually. It is also a disease which existing knowledge—if deployed with sufficient force behind it—could eradicate as surely as vaccination and public health measures have virtually abolished such diseases as diphtheria and cholera from many countries where they were once rife.

Such a project, or any other that would tax the combined resources of all to the utmost, deserves the fullest support. It could both capture the imagination of the public and convincingly demonstrate that the applications of science are not a matter of chance—when evil and good results have equal chances of emerging—but can be deliberately steered into beneficent channels if mankind so wills. It would, moreover, be a major contribution to world peace. While the underlying causes of war so far defy analysis, one important factor undoubtedly is that man's restlessness demands an outlet. War is one such outlet: vast international projects such as science has made both possible and profitable might well be an effective alternative.

Thermonuclear power and the pinch effect

J. E. ALLEN

At present power-producing nuclear reactors utilize the fission process. A possible alternative, however, is offered by the fusion of hydrogen isotopes. Some discussion of the fusion process is given, and it is shown that fusion power will probably be obtained from a thermonuclear reaction if it can be obtained at all. The extreme physical conditions that must be developed in a power-producing reactor are discussed, and the self-magnetic pinch effect, which offers one of the more promising methods of achieving these conditions, is described in some detail. The instabilities that arise in pinched discharges are described, together with the means used to combat them, and an outline is given of methods that can be used to deliver energy to a pinched discharge, together with possible ways of extracting energy.

In a recent issue of ENDEAVOUR Sir Christopher Hinton [1] described the nuclear reactors which are now being built to produce electrical power. These utilize the fission reaction that takes place among heavy elements. In this reaction a heavy nucleus is split into fragments, with an accompanying release of nuclear energy. Another way of obtaining nuclear energy, however, is by the fusion of light elements. In both these reactions the mass of the products is less than the mass of the original materials, and the energy produced is the equivalent of the matter that has vanished. The latter process is operative at the centre of the Sun, where hydrogen atoms are fused together to form helium, and also in the hydrogen bomb. It has been estimated that the conversion rate at the centre of the Sun is about 800 million tons of hydrogen per second.

The probability of a fusion reaction taking place, as opposed to the more usual elastic collision, is known, and it is found that by far the most probable fusion reactions are those involving the hydrogen isotopes deuterium and tritium (hydrogen 2 and hydrogen 3 respectively). Of these, deuterium is plentiful on the Earth, particularly when compared with the fissile isotope of uranium, since it forms about one part in 5000 of natural hydrogen. Thus the oceans contain a virtually inexhaustible supply of fuel if an efficient way of 'burning' deuterium can be found. Tritium, on the other hand, does not occur naturally.

In addition, if deuterium can be used as a fuel, no appreciable amount of radioactive waste is likely to be produced. This would represent a great advantage over fission power, where the disposal of radioactive waste will become increasingly difficult. It is considerations such as these, together with the intrinsic appeal of the problem, that have aroused world-wide interest in the possibility of using the fusion reaction as a source of power.

THE FUSION REACTION

Protons, the nuclei of ordinary hydrogen, have a low probability of fusing in an encounter, but the nuclei of deuterium and tritium are much more likely to fuse. The following reactions may take place in a collision between two deuterons:

$$H^2 + H^2 \rightarrow He^3 + \text{neutron} + 3{\cdot}3\ \text{MeV} \quad \ldots.(1)$$

$$H^2 + H^2 \rightarrow H^3 + H^1 + 4\ \text{MeV} \quad \ldots.(2)$$

If a deuteron collides with a triton (the nucleus of a tritium atom) they may undergo the reaction

$$H^2 + H^3 \rightarrow He^4 + \text{neutron} + 17{\cdot}6\ \text{MeV} \ldots.(3)$$

The first two reactions are equally probable; the third is of interest because it is easier to achieve, and also because it involves a reaction product of the second reaction. Thus if the tritium formed in reaction 2 is 'burnt' as soon as it is formed, 24·9 MeV is obtained from the reaction of five deuterons.

Figure 1 shows graphs of the 'cross sections' for the fusion reactions between two deuterium nuclei, and between a deuterium nucleus and one of tritium. The cross section can be thought of as the target area which must be hit if the reaction is to take place, and is a measure of the probability of the reaction. The deuterium-tritium (D-T) reaction has a greater cross section than the fusion reaction of two deuterium nuclei (called a D-D reaction), but tritium, as mentioned already, does not occur naturally. It can be 'bred' by capturing

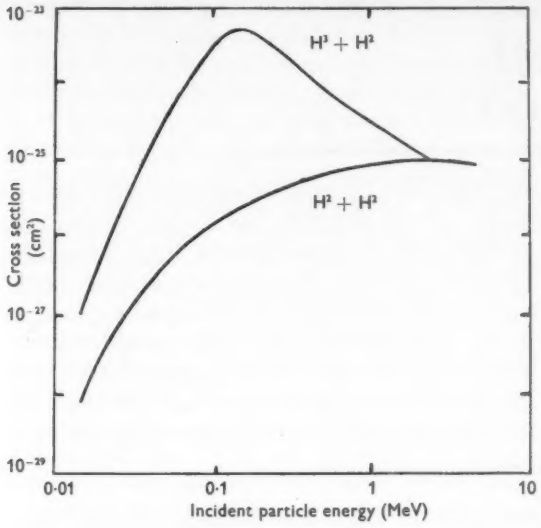


FIGURE 1 – *Cross sections for D-D and D-T reactions. The lower curve refers to either of the D-D reactions, which have an almost identical cross section.*

neutrons (from the D-D reaction) in the light isotope of lithium, Li^6,

$$Li^6 + \text{neutron} \rightarrow He^4 + H^3 + 4{\cdot}8 \text{ MeV}$$

but lithium is much less abundant than deuterium. It is, therefore, highly desirable to use the D-D reaction for energy production if possible.

The most probable consequence of an encounter between two nuclei is an elastic collision, due to the Coulomb repulsion between the two charged particles. This is true even at energies up to 0·1 MeV, so that there will be a large number of elastic collisions for a small number of nuclear reactions and the velocities of the nuclei will quickly become 'randomized'. This strongly suggests that efficient fusion power will be obtained from 'thermonuclear' reactions, that is, reactions in which the nuclei have a thermal distribution of velocities, similar to that possessed by the molecules of an ordinary gas.

Whether or not two colliding particles fuse depends on their relative velocity. In a hot, completely ionized gas the nuclear reactions take place in collisions between the most energetic particles. W. B. Thompson has shown [3] that the reaction rate can be written in the form $r = n_a n_b \langle \sigma v \rangle$ for reactions between unlike particles and $r = \frac{1}{2} n^2 \langle \sigma v \rangle$ for reactions between like particles. Here n_a, n_b, and n are particle densities (number/cm³), and $\langle \sigma v \rangle$ is the product of the cross section and the relative velocity averaged over the Maxwellian distribution of velocities. The reaction probability $\langle \sigma v \rangle$ depends only on the temperature, and figure 2 shows the curves computed by Thompson for the D-D and D-T reactions. Thompson also considered a number of other reactions, but concluded that the D-D and D-T reactions were the only significant ones for temperatures less than 10^9 °K.

In the case of the D-D reaction (taking both reactions (1) and (2) together) 66 per cent of the reaction energy is given to the charged reaction products, on the average, and 34 per cent is given to the neutrons. In the case of the D-T reaction only 20 per cent of the reaction energy is given to the charged products and 80 per cent to the neutrons. These figures are of interest, since the charged products might remain in the plasma but the neutrons will always escape and deposit their energy elsewhere, and this distribution of energy is one of the factors in the calculation of the conditions under which a thermonuclear reactor will produce more energy than it consumes.

REQUIREMENTS FOR A POWER-PRODUCING REACTOR

It is not known at present what form a thermonuclear reactor will take, or indeed whether one can be built at all. Nevertheless, it can be shown that temperatures considerably greater than 3×10^7 °K must be achieved if a reactor is to become a practicable proposition. At such temperatures the hydrogen isotopes are completely ionized, and the problem resolves itself into a study of the physical properties of a completely ionized gas. Such a gas

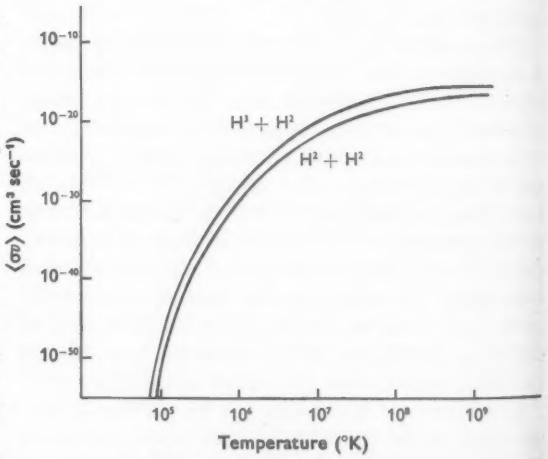


FIGURE 2 – *Plot of the reaction coefficient $\langle \sigma v \rangle$ which is used in the calculation of thermonuclear reaction rates. The lower curve refers to the combined D-D reaction.*

differs from an ordinary one in that it interacts with electric and magnetic fields. In fact this interaction offers a possible solution to the problem, since one might be able to hold the gas away from the walls of its containing vessel by electromagnetic forces, so that conduction losses are negligible, and then heat it to literally astronomical temperatures.

An ionized gas has been referred to as a 'fourth state of matter' to emphasize its peculiar properties: in this paper it will be referred to as the 'plasma', following the nomenclature adopted by I. Langmuir and L. Tonks [2] in their pioneer work on the subject. A plasma in which most of the atoms are stripped of their electrons can emit little energy as line spectra, but a continuous spectrum known as 'bremsstrahlung' is emitted. A free electron has a finite probability of making a spontaneous transition from one hyperbolic orbit round a nucleus to another hyperbolic orbit of less energy, with the emission of a photon of frequency ν, where $h\nu$ is the energy difference between the two orbits. Bremsstrahlung radiation consists of the photons emitted in these 'free-free' transitions. Most of these photons escape from the gas without absorption, and they constitute an unavoidable energy loss. G. Cillié [4] has calculated the power emitted in this way from nebulae. His formula is applicable here, and reads

$$p = 1{\cdot}42 \times 10^{-34} n_1 n_2 Z^2 T_1^{\frac{1}{2}} \text{ watts/cm}^3$$

where n_1 and n_2 are the electron and ion densities respectively, Z is the ionic charge, and T_1 is the temperature of the electrons (which are assumed to have a Maxwellian energy distribution). Provided the ion densities are not too small, n_1 is nearly equal to Zn_2 throughout the greater part of any ionized gas (i.e. the plasma as a whole is neutral), so that the expression for the power loss can be written in the form

$$p = 1{\cdot}42 \times 10^{-34} n_2^2 Z^3 T_1^{\frac{1}{2}} \text{ watts/cm}^3$$

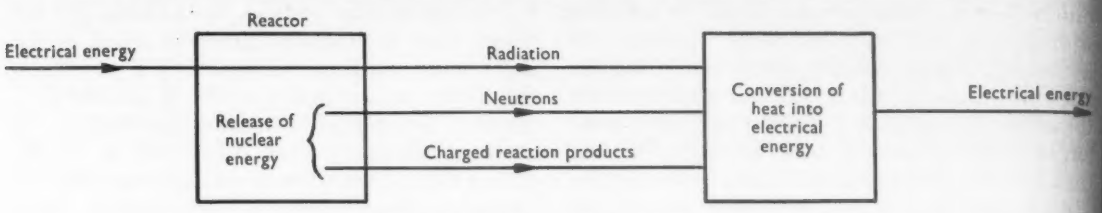
The radiation differs from that of a black body because the conditions of thermodynamic equilibrium are not approached. In the latter case every process occurs at the same rate as its converse (this is the principle of detailed balancing), whereas in the present case the absorption is negligible compared with the emission, because the amount of matter is small. The factor Z^3 in the equation shows that the radiation loss is more serious for highly charged nuclei, and for this reason only the elements of low atomic number are suitable as fuel for the thermonuclear reactor.

By comparing this formula with the figures for the thermonuclear yield J. D. Lawson [5] has shown that the radiated power is equal to the latter at a temperature of about $1{\cdot}5 \times 10^8$ °K for the D-D reaction and 3×10^7 °K for the D-T reaction. For comparison, the temperature at the centre of the Sun is about $1{\cdot}5 \times 10^7$ °K. In the case of the D-D reaction it was assumed that the tritium was burnt as soon as it was formed. These figures give some idea of the temperatures required, but the economical operation of a reactor demands even higher temperatures.

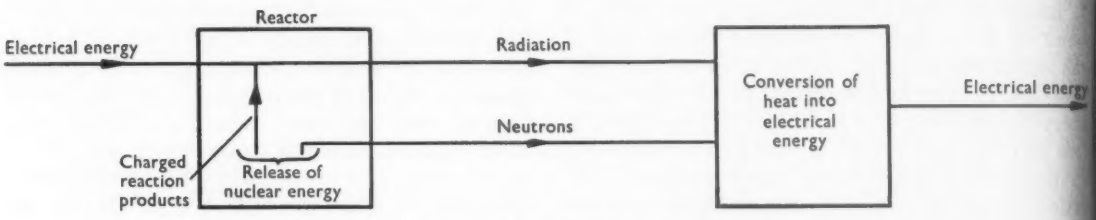
If the reaction products escape from the plasma and deposit their energy as heat which is then transformed into electrical energy by conventional means (as shown in figure 3*a*), the nuclear energy released must be at least twice the radiation loss, because the efficiency of conversion is only about 33 per cent. The lowest operating temperatures, calculated on this basis, are 2×10^8 °K for the D-D reaction and 3×10^7 °K for the D-T reaction. If the reactor operates in pulses, as seems likely, an appreciable amount of energy is required to heat the plasma to the appropriate temperature at the beginning of each pulse. For economical operation the electrical energy that is generated must exceed the sum of the radiated energy and the energy required to heat the plasma. Ignoring the dissociation and ionization energies and the losses during the heating time, Lawson showed that on this basis nt must be greater than 10^{16} for the D-D reaction, where n is the ion density (number/cm^3) and t is the time in seconds during which the plasma is hot. Thus if $n = 10^{16}$ deuterons/cm^3, t must be greater than 1 second. It is evident that long times and high ion densities are required in addition to very high temperatures. In the case of the D-T reaction conditions are easier, but still severe, and nt must exceed 10^{14}.

It is possible that the charged reaction products will remain in the plasma (as illustrated in figure 3*b*), since they are affected by electric and magnetic fields, but the neutrons will always escape and release their energy as heat when absorbed. Lawson states that the nt values are almost unaltered if the charged reaction products are contained.

At a certain temperature (3×10^8 °K for D-D and 5×10^7 °K for D-T) the nuclear energy associated with the charged reaction products is equal to the radiation loss. There is a possibility that, at higher temperatures than this, part of the nuclear yield may be fed directly into the electrical supply. The mechanism of this energy transfer will be discussed further in a later paragraph.



Case (*a*). *System in which the charged reaction products escape.*



Case (*b*). *System in which the charged reaction products are contained.*

FIGURE 3 – *The generation of electrical energy from a controlled thermonuclear reaction. During certain cycles of operation the electrical energy in case* (*b*) *may flow from the reactor to the electrical supply.*

These, then, are the ion densities and temperatures involved. One of the more promising methods of confining a plasma can now be considered.

THE PINCH EFFECT

A current-carrying conductor tends to be compressed under the action of its own magnetic field, as illustrated in figure 4. This is known as the pinch effect. It is due to the interaction of the current and the field, which produces an inwardly directed radial force, following Fleming's left-hand rule. Experiments on this effect, using liquid mercury, were reported by E. F. Northrup in 1907 [6]. The theory of the effect in an ionized gas has been developed by W. H. Bennett [7], L. Tonks [8], and others. In 1937, P. L. Bellaschi and R. C. Mason [9] stated that self-magnetic effects were present in the high-current atmospheric discharges which they were studying in the laboratory, and this type of discharge has been further studied by later workers [10–12].

From the thermonuclear point of view it is desirable to ionize the gas within a low-pressure discharge tube, and then to pinch it inwards, away from the walls of the tube. This effect was demonstrated in 1951 by S. W. Cousins and A. A. Ware [13], who were working under the direction of Sir George Thomson.

It can be shown that if a steady state is attained, wherein the pressure of the plasma is balanced by magnetic forces, then

$$I^2 = 4NkT \qquad \ldots\ldots(4)$$

where I is the discharge current in e.m.u., N is the number of ions per unit length, k is Boltzmann's constant, and T is the temperature of the electrons or ions (assumed equal). It should be mentioned that although the magnetic force acts primarily on the electrons, since they carry the current, the electrons and ions are closely bound together by space-charge forces, as any appreciable separation of the electrons and ions would produce enormous

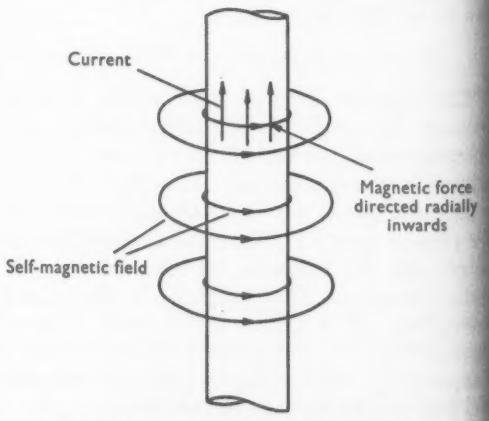


FIGURE 4 – *The self-magnetic pinch effect.*

FIGURE 5 – *A streak photograph of a helium discharge in ZETA (see below) which lasts for about three milliseconds. The photograph was obtained while rapidly moving the film during the exposure, thus providing time resolution. The left-hand end of the lowest line marks the beginning of the pulse and the two fine lines indicate the diameter of the discharge tube (the thick black lines are due to obstructions blocking the window).*

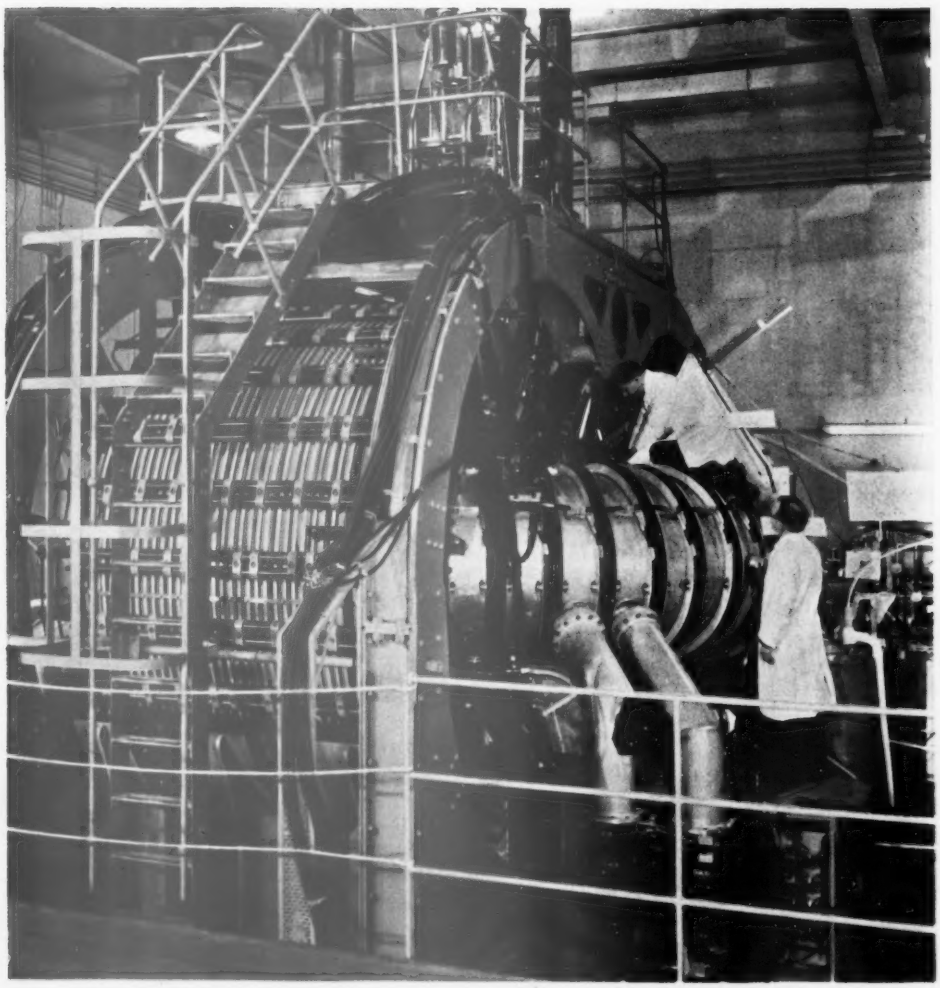


FIGURE 6 – *ZETA (Zero Energy Thermonuclear Assembly), a large-scale apparatus which has been built at the Atomic Energy Research Establishment, Harwell.*

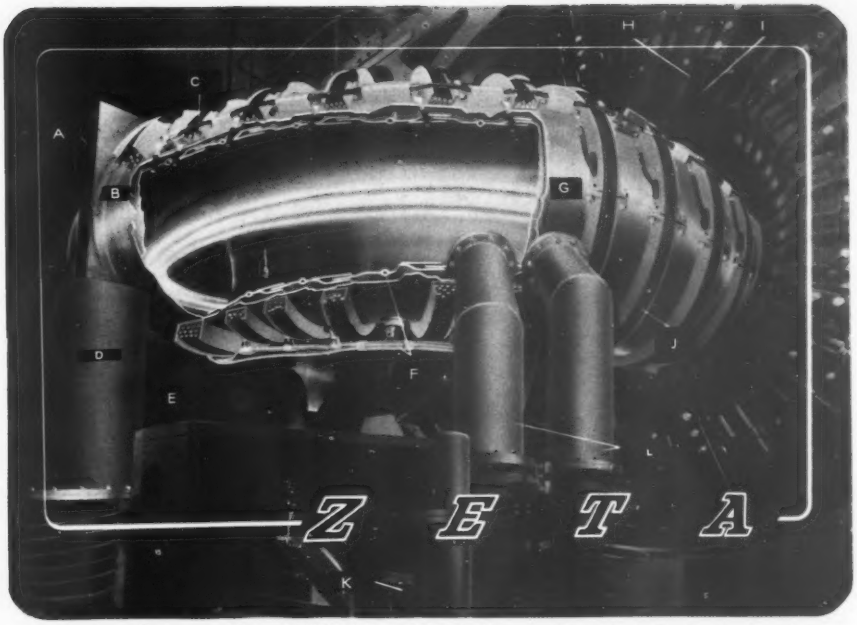


FIGURE 7 – *Cut-away view of ZETA, in which the letters refer to the following parts. A, viewing window; B, liners (of which there are 48), each insulated from its neighbours by insulators of polytetrafluorethylene C; D, liquid-air trap for the vacuum system; E, vacuum pumping manifold; F, water-cooling pipe for liner; G, torus body; H, primary windings of the transformer; I, bias windings; J, axial field windings; K, water-flow relay; L, vacuum pumps.*

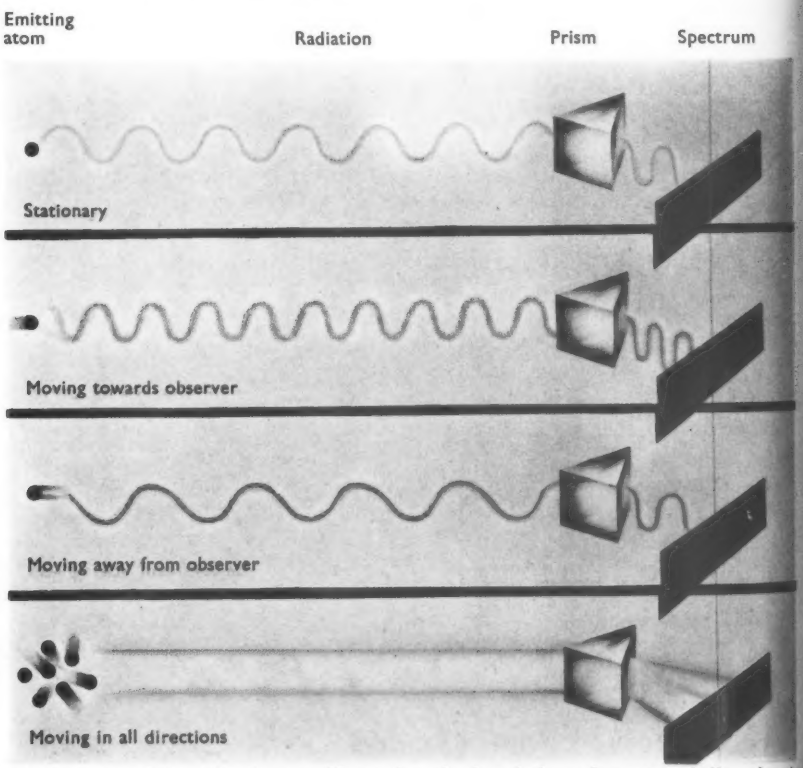


FIGURE 8 – *Schematic diagram illustrating the broadening of a spectral line due to thermal motion.*

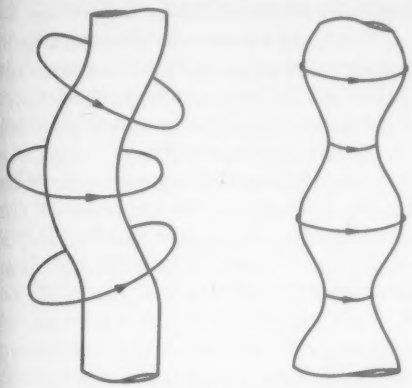


FIGURE 9 – *Instabilities of the pinched discharge.*

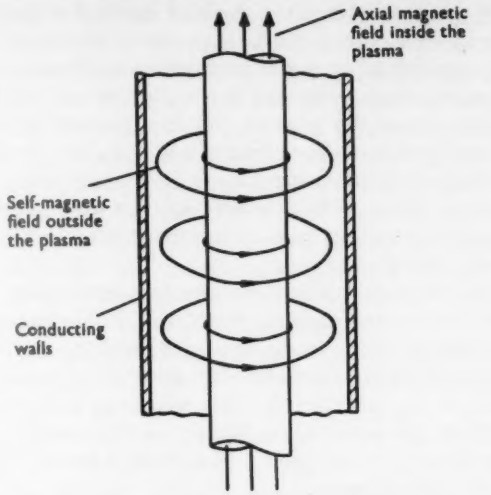


FIGURE 10 – *The stabilized pinched discharge.*

electric fields. A very small separation does occur, and the associated electrostatic field holds in the positive ions.

Heat losses to electrodes can be avoided by inducing the current in a ring-shaped discharge tube. The induced electric field is provided by linking the ring with a changing magnetic flux. Such a device is of course a transformer in which the plasma forms a single-turn 'secondary winding'. The transformer may or may not be designed with an iron core.

The main obstacle that has been encountered in experimental work on the pinch effect is the fact that the system is inherently unstable, as was shown by S. Lundquist [14]. Two possible modes of instability are illustrated in figure 9. These are usually referred to as 'kink' and 'sausage' instabilities respectively. In both cases the magnetic field strength is increased at certain points and decreased at others, and the resultant magnetic force tends to increase the deformation.

These instabilities and others have been discussed theoretically by M. D. Kruskal and M. Schwarzschild [15], and have been encountered in the laboratory by G. G. Timofeeva and V. L. Granovskii [16], I. V. Kurchatov and his colleagues [17], and others [18–20]. In general a pinched discharge will collapse—that is become constricted, as desired—and then undergo some radial oscillation, finally becoming unstable. Reproducible measurements can be made during the collapse time, which is determined essentially by the inertia of the ionized gas [17, 21, 22].

Instabilities are most undesirable, since conduction losses become appreciable if the plasma strikes the wall of the containing vessel. In addition, impurities are driven off the wall into the discharge, and these greatly increase the radiation loss, and also increase the value of N in equation (4), thus lowering the temperature T. For these reasons it is important to consider how to stabilize the discharge.

CONTROL OF INSTABILITIES

The stabilizing effect of an additional magnetic field in the axial direction was first pointed out by Lundquist [14]. More recently the subject has been treated in greater detail by V. D. Shafranov [23], M. Rosenbluth [24], and R. J. Tayler [25]. These investigators have discovered a theoretically stable configuration which is illustrated in figure 10. The stability is achieved by including an axial magnetic field inside the plasma, restricting the self-magnetic field to the region outside the plasma, and by the presence of conducting walls. In this case the effective tension of the axial magnetic field is operative, which can be shown as follows. The magnetic field lines in a perfect conductor (the model used in these calculations) are 'frozen' into the conductor, since any relative motion between the field lines and the conductor generates an electric field in the latter, and this is contrary to the assumption of a perfect conductor. Thus if the plasma is deformed, so are the axial lines of force, and the tension tends to restore the plasma to its original position. Experimental work on this method was first reported by A. L. Bezbatchenko, I. N. Golovin, D. P. Ivanov, V. D. Kirillov, and N. A. Yavlinsky [26], who reported a delay in the onset of instabilities when the additional magnetic field was employed.

The sausage type of instability can be overcome

by this tension but not the kink instability shown in figure 9, which can be suppressed, however, by the combined action of an internal field and conducting walls, provided the walls are not too far away from the plasma. If the plasma moves towards the wall, currents are induced in the latter, and, following Lenz's law, these tend to oppose the motion to which they are due, i.e. the magnetic force is such as to restore the discharge to its initial position.

In these calculations the currents were assumed to flow in an infinitely thin 'skin'. Calculations which are being made at present will show what skin depths are allowable if stability is to be maintained. In practice the two magnetic fields will diffuse into each other, due to the finite electrical conductivity, and after a certain time instabilities will begin to grow.

Once started, the instabilities grow rapidly, the time involved being comparable with the transit time for a sound wave travelling across the plasma.

THE ORTHOGONAL PINCH EFFECT

While no attempt is made here to discuss plasma containment from a general point of view, one other case may be mentioned—the 'orthogonal' pinch effect. In this the current and magnetic field shown in figure 4 are both rotated through 90°. The magnetic lines of force become straight, the lines of current flow become circles, and the resulting magnetic force is still directed radially inwards. An advantage of this system is that it is more stable than that producing the ordinary pinch effect, but it suffers from the disadvantage that it cannot be bent round to form a ring, as field variations would then destroy the equilibrium.

The circulating current must be induced in the plasma by changing the current in an external coil. For this reason the method is often referred to as compression by an external current. This is somewhat misleading, however, since currents must flow both in the plasma and in external conductors in either 'pinch-effect' system.

THE HEATING PROBLEM

The gas must not only be held away from the walls of the containing vessel, but it must be heated to a very high temperature. The energy can be supplied either by Joule (resistance) heating or by compression.

The energy associated with Joule heating is given primarily to the electrons, which carry the current. In due course the energy is shared with the positive ions in collision processes. The electrons must make many collisions before the ions are heated, however, because an electron can lose only a small fraction of its energy in an encounter with a massive positive ion. At high temperatures the necessary 'relaxation time' may be quite long, depending on the ion density [27].

Partly because this heating time is long, there is a considerable interest in the mechanical compression of a plasma. This is effected by the magnetic pinch force as the discharge collapses. It can easily be shown that the mechanical power delivered to a pinched discharge is $\frac{1}{2}I^2dL/dt$, where dL/dt is the rate of change of inductance of the discharge. In certain cases this power may exceed that associated with Joule heating.

The behaviour of a plasma when subjected to a compressive force depends on (*a*) whether the compression is fast or slow (compared with sound transit times across the plasma), (*b*) whether an internal magnetic field exists, and (*c*) whether collisions take place between particles. Although it might be argued that collisions must take place if the gas is to be heated, i.e. if the particles are to be given the random motion associated with a high temperature, these collisions might take place after the compression of the discharge.

If collisions take place frequently the plasma may be compressed adiabatically or shock-heated, depending on whether the compression is slow or rapid. If collisions are rare and the gas is slowly compressed, the particles are accelerated perpendicularly to the internal field. The magnetic flux threading any particle orbit increases, and there is an induced electric field (according to Faraday's law of induction) which accelerates the particle round its orbit. The net result can be thought of as a sort of adiabatic compression in which only two degrees of freedom are involved. It can be shown that the energy of the particles is directly proportional to their density (number per cm^3) and that the latter varies directly as the magnetic field strength. If, on the other hand, collisions are rare and the compression is fast, strong collision-free hydromagnetic waves are generated. These form another subject for research at the present time [28].

It is conceivable that a plasma might receive enough energy in a single compression to raise it to a very high temperature. In certain cases, however, repeated compression and expansion will continuously heat the gas. One such case has been described by A. Schlüter [29], who discussed the situation in which the collision frequency is comparable with the frequency of compression.

DIRECT CONVERSION OF NUCLEAR ENERGY INTO ELECTRICAL ENERGY

The electromechanical power that is supplied to a collapsing discharge is $\frac{1}{2}I^2dL/dt$; the mechanism can be thought of as an 'electric motor' action in which electrical energy is transformed into mechanical energy. By reversing the sign of dL/dt, i.e. by allowing the discharge to expand, the electromechanical power becomes negative, which means that energy is flowing from the discharge to the electrical supply. The 'motor' action is thus replaced by a 'dynamo' action.

If a thermonuclear reaction takes place in a pinched discharge and the charged reaction products are contained, as shown in figure 3*b*, an expansion of the plasma will result in a direct transfer of nuclear energy to the electrical supply, provided that the radiation loss is not too great. The temperature must therefore exceed 3×10^8 °K in the case of the D-D reaction or 5×10^7 °K in the case of D-T reaction, since only at these temperatures does the nuclear power associated with the charged reaction products exceed the radiation loss.

The nuclear energy which is imparted to the neutrons, on the other hand, can only be abstracted as heat. Thus, even if direct conversion is used in the future, it will almost certainly be used in conjunction with conventional generating equipment so as to use this neutron energy.

PRELIMINARY RESULTS FROM ZETA

The results that have been obtained with ZETA (Zero Energy Thermonuclear Assembly) have recently been reported [30]. This is a large experimental device which has been built at the Atomic Energy Research Establishment, Harwell (figure 6). (The phrase 'Zero Energy' is used because the energy produced in the fusion reaction is negligible compared with the energy input.) The discharge tube is an aluminium torus of 1 metre bore and 3 metres mean diameter. The gas inside this ring forms the secondary of a large pulse transformer and carries unidirectional current pulses, of maximum value 200 kA, which last for several milliseconds. This pulse length is much longer than those used by previous workers: the importance of long containment times was emphasized in a previous paragraph. An 'axial' magnetic field is provided by coils wound on the torus and can be varied up to 400 gauss.

It was found that neutrons were emitted from ZETA, and 'ion temperatures' up to 5×10^6 °K were measured spectroscopically. The method of determining the temperature can be explained as follows. The frequency of a wave, as measured by a stationary observer, depends on the relative velocities of the source and the observer; this is the well-known Doppler effect. The same effect also applies in the case of the emission of light by a moving atom: the spectral line is shifted in frequency by an amount which depends on the velocity of the emitting atom. Some of the atoms in a hot gas will be moving away from the observer and some towards the observer. The net result is that the line is broadened, as illustrated in figure 8. If other broadening processes can be neglected, measurement of the line width provides a measure of the temperature.

Obviously, spectral lines emitted by the positive ions must be used if the ion temperature is required. This method cannot, unfortunately, be used directly in a hydrogenous plasma, since, as the hydrogen atom has only one bound electron, its positive ions have none and cannot emit a line spectrum. For this reason lines emitted by impurity ions (oxygen and nitrogen) have been used to measure the ion temperature in ZETA.

Both the detection of neutrons and the observations of Doppler broadening show that energetic ions exist in the discharge. The latter measurements, however, refer to an average ionic velocity in the 'line of sight', and further work is necessary to determine the extent to which the ionic velocities have been 'thermalized'. Also the spectroscopic measurements were made with impurity ions, and their energy may differ from that of the deuterons.

An important point to be remembered is that neutrons can be produced by other mechanisms than a thermonuclear reaction. American workers [19, 20] have found neutrons with a non-isotropic distribution, showing that some kind of accelerating mechanism is operative, a mechanism that may be bound up with the growth of the sausage type of instability shown in figure 9 [20]. For these reasons the Harwell team was cautious and did not claim that a thermonuclear reaction had definitely been produced.

Assuming that the temperature measurements are valid, L. Spitzer [31] holds the view that electron-ion collisions are inadequate to explain the observed rate of heating, and he suggests that co-operative phenomena may be important. It is clear that, while the recent results obtained by P. C. Thonemann's team and by others [32–34] are most encouraging, more experimental work is necessary before it can be definitely stated that a

thermonuclear reaction has been obtained in the laboratory.

CONCLUSIONS

The study of controlled thermonuclear reactions is in an active and therefore somewhat fluid state. It is not known at present which is the best way to confine a plasma and to heat it to astronomical temperatures. Emphasis has been laid on the pinch effect in this paper, but some other approach may be used. The basic problems which have been discussed with reference to the pinch effect, however, also apply in some measure to other systems.

It is not possible to estimate (but only to guess) the time which will elapse before the arrival of thermonuclear power. What is certain is that the physical properties of a plasma will be elucidated by research in this field and that technical applications will follow in due course. From the scientific point of view, experimental work on plasma is of great interest, since much of the universe exists in this 'fourth state of matter'.

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The scientific work of Emanuel Swedenborg

HERBERT DINGLE

Emanuel Swedenborg is nowadays known almost solely as a religious writer and a mystic, although he was also a diligent student of science with a fertile imagination and was responsible for many original interpretations. A number of aspects of his scientific work, and the reasons for its neglect by both contemporary and more recent scientists, are studied here.

'Everything of importance', wrote A. N. Whitehead, 'has been said before by somebody who did not discover it.' He was referring to those whose minds are too small to comprehend their acts. There are, however, those whose acts are too large for the minds of their contemporaries, and they also may say things of importance which cannot be said thus to have been discovered in the customary sense of the word.

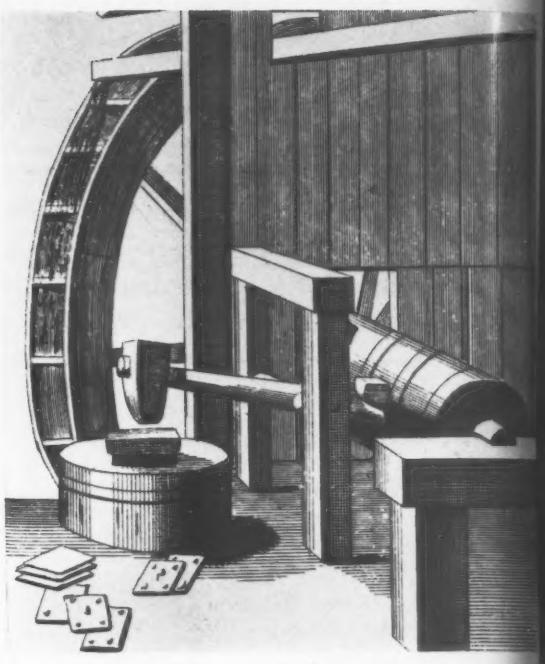
Emanuel Swedenborg was born in Stockholm on 29th January 1688, but the greater part of his early life was spent in Uppsala, where his father, a Lutheran bishop, had made his home. His performances during his schooldays appear to have been creditable but otherwise undistinguished. At the age of 22 he began an extended period of foreign travel, spending nearly two years in London and Oxford and another three years in various European cities. In England he was chiefly interested in the study of mathematics and astronomy. 'I study Newton daily', he wrote, 'and am very anxious to see and hear him.' There is no record that he did so, but he made the acquaintance of Flamsteed, Halley, and various other members of the then young Royal Society. Among other pieces of original work he devised a method of determining longitude from observations of the Moon.

FIGURE 1 – *Emanuel Swedenborg.*

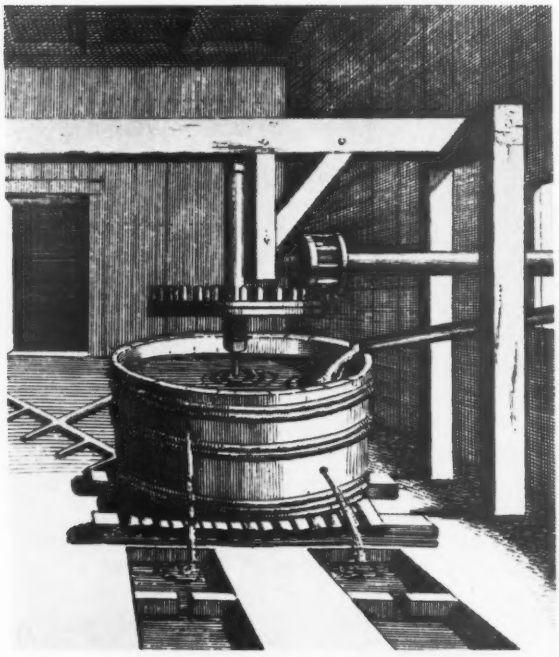
From Paris we have an interesting letter written in 1713, which records that 'Between the mathematicians here and the English there is great emulation and jealousy. Halley, of Oxford, told me that he was the first who examined the variation of the pendulum under the equator; they keep silence about this here; the astronomers here also maintain that Cassini's paper was written before Halley made his expedition to the Island of St Helena, and so forth.'

During these years his inventive faculty was very active, and a list of projects, including proposals for submarines and flying machines, which he drew up suggests a mind like that of Leonardo. There is no record, however, of any construction.

Swedenborg returned to his native land in 1715, full of enthusiasm for the promotion of scientific research in what was then a relatively backward country in this respect. Though greatly interested in mathematical studies, his inclinations appear to have been predominantly practical, and in commenting on the apathy of Swedish mathematicians concerning a proposal to establish an astronomical observatory at Uppsala he wrote: 'The mathematicians . . . have lost all energy and desire to follow up so clever a design, as the one you pointed out to them of the building of an astronomical observatory. It is a fatality with mathematicians that they remain mostly in theory. I have thought that it would be a profitable thing if to ten mathematicians there was added one thoroughly practical man, by whom the others could be led to market; in which case this one



FIGURES 2 *and* 3 – *Water-driven trip hammers. These, and figures* 4 *and* 5, *are from Swedenborg's* Regnum subterraneum, 1734, *a finely illustrated work.*



FIGURES 4 *and* 5 – *Extraction of copper. The first shows enrichment of copper ore by flotation, and the second, apparatus used for assaying copper ores. At the top of this figure a furnace is shown, while the lower diagrams show* (*left*) *steam being blown through the roasted ore and* (*right*) *separation by flotation.*

man would gain more renown and be of more use than all the ten together.'

While awaiting a permanent post he started a scientific and technical journal, entitled 'Daedalus Hyperboreus', which, though it received the approval of the King, the famous Charles XII—and, still more remarkable, was read and discussed by him—ceased with the sixth number owing to lack of funds. The young enthusiast, however, was appointed by the King late in 1716 to the post of 'Extraordinary Assessor' at the Board of Mines, which turned out to be so much to his liking that two years later he refused an offer of a professorship of astronomy in the University of Uppsala on the grounds that he could be of more use to his country where he was and that by his tastes and turn of mind he was 'led to mechanics, and will be in future to chemistry', in which subjects 'our Board is noted for having assessors who know very little'.

The death in November 1718 of Charles XII, who had shown great interest in his schemes, was a sad blow, for in Sweden generally they were regarded with almost complete indifference. In the summer of 1721 he started a year's tour of Holland and the German States in order to study the mines and manufactures of those countries. Notwithstanding his meagre success in persuading his countrymen of the importance of such studies as he was pursuing, he was appointed in 1724 as an ordinary assessor to the Board of Mines—that is, to a normal rather than a supernumerary position—and in the immediately succeeding years he published a number of scientific works which won him a European reputation. These were predominantly technical in character, but about 1734 his early interest in the more theoretical aspects of science seems to have reawakened, and from then onwards he gave much attention not only to such fundamental problems in the physical sciences as the ultimate structure of matter, the basic laws of mechanics, the origin of the solar system, and the nature of magnetism, but also to equally fundamental questions in anatomy, physiology, and what would now be termed psychology.

It would be a fascinating study for those with the necessary qualifications, if any such exist, to seek an interpretation in psychological terms of the progress of Swedenborg's dominant interest from the most practical aspects of mining, through chemistry, physics, the mathematical foundations of mechanics, the fundamental principles of physiology, and the nature of the soul, to the ultimate mystical experiences for which he is now chiefly known. Here we can only record the facts.

In the field of practical science his contributions were both topical and profound. The improvements that he introduced into mining and metallurgy of Sweden bear witness to his ability, enterprise, and patriotism, but it is in other directions that his essential originality is better manifested. According to his great fellow-countryman, Berzelius, he was the first—in a paper published in 1719—to point out that the Swedish coast was rising, and the evidence he gives for this involves principles now familiar enough but then new. Sylvanus Thompson ascribes to him the invention of the first mercurial air-pump. Dumas, the French chemist, credits him with originating the conception, basic in the modern science of crystallography, of constructing crystalline forms by grouping spheres, and although there is evidence that this possibility occurred earlier to Kepler, it seems unlikely that the German mathematician associated it with the minute structure of matter as did Swedenborg. It is often impossible to say with certainty who was the first to conceive a particular idea, but these testimonies (and others could be added) from men eminent and well informed in their own subjects make it clear that the fertility of Swedenborg's imagination, of which there is first-hand evidence in his theoretical work, was also operative in the realm of practical invention.

Without losing its interest in this, however, his mind automatically sought for the principles that determined what was and what was not possible in nature. His *Principia*, which forms the first part of his *Opera Philosophica et Mineralia*, published at the expense of his friend the Duke of Brunswick-Lüneburg in 1734, is a treatise as fundamental in character as Newton's *Principia*. Both writers propose principles of motion based on primary philosophical concepts, but whereas Newton changed the medieval postulate (partly accepted by Galileo) that perfect natural motion was circular to the postulate that it was rectilinear, Swedenborg preferred the spiral form—which may be regarded as a combination of a circular with an outward linear motion. On this basis he constructed a theory of the structure and movements of matter, leading to a view of the origin of the solar system identical in many respects with that later propounded by Laplace and generally known as the nebular hypothesis. His nomenclature, which has not come into general use, naturally gives his writing a flavour now chiefly familiar to us in the productions of woolly-minded cranks, but

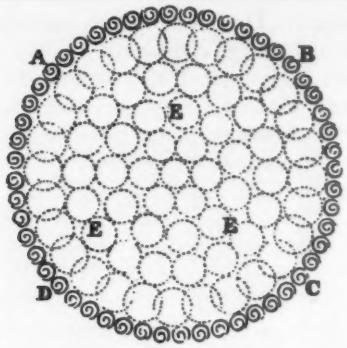


FIGURE 6 – *Swedenborg's conception of 'the first elementary particle' ('atom' in our terminology), in which the circles EE represent 'actives' and the surrounding spirals 'finites'. These have remarkable resemblances to our nucleons and electrons.*

when the inevitable initial repulsion has been overcome it is seen to express clear and penetrating thought. The course of mechanical evolution is worked out in great detail, and the principles are applied to the explanation of magnetism. Here Swedenborg clearly anticipates the modern molecular theory of magnetism, and gives diagrams which could be reproduced with propriety in present-day textbooks on the subject. He brings his theoretical views into close relation with experiments, particularly with those then being made by Musschenbroek, and shows how they explain the phenomena that were progressively being discovered.

The conceptions explored in the *Principia*, though fundamental to physical science, were not yet fundamental enough for Swedenborg, to whom the distinction between physical science and knowledge as a whole was of little significance. His attention was next directed to the working of the human organism, first the body, and then the soul. Well aware that success was to be achieved not by speculation but by prolonged study of the facts and reflection upon them, he sought and obtained, in the years following the publication of the *Principia*, extended leave of absence, and travelled in various countries, absorbing the knowledge of anatomy and physiology available there. Though here, as in the physical sciences, he strongly insisted on experiment as providing the basis of knowledge, and indeed performed a few experiments himself, he relied in this work chiefly on the observations of others. 'Here and there', he wrote when publishing the results of his deliberations, 'I have taken the liberty of throwing in the results of my own experience; but this only sparingly, for on deeply considering the matter, I deemed it best to make use of the facts supplied by others. Indeed there are some that seem born for experimental observation, and endowed with a sharper insight than others, as if they possessed naturally a finer acumen; such are Eustachius, Ruysch, Leeuwenhoek, Lancisi, etc. There are others again who enjoy a natural faculty for contemplating facts already discovered, and eliciting their causes. Both are peculiar gifts and are seldom united in the same person. Besides I found when intently occupied in exploring the secrets of the human body, that as soon as I discovered anything that had not been observed before, I began (seduced probably by self-love) to grow blind to the most acute lucubrations and researches of others and to originate the whole series of inductive arguments from my particular discovery alone; and consequently to be incapacitated to view and comprehend, as accurately as the subject required, the idea of universals in individuals, and of individuals under universals. Nay, when I essayed to form principles from these discoveries, I thought I could detect in various other phenomena much to confirm their truth, although in reality they were fairly susceptible to no construction of the kind. I therefore laid aside my instruments and restraining my desire for making observations, determined rather to rely on the researches of others than to trust to my own.'

The biological results were published in two works, *Œconomia regni animalis* (1740) and *Regnum animale* (1744–45). Here *anima* bears the general meaning of the soul, and only in a secondary sense refers to what we call the animal kingdom. In the introduction to the latter work he wrote: 'I intend to examine, physically and philosophically, the whole anatomy of the body. . . . The end I propose to myself in the work is a knowledge of the soul; since this knowledge will constitute the crown of my studies. . . . In order therefore to follow up the investigation, and to solve the difficulty, I have chosen to approach by the analytic way; and I think I am the first who has taken this course professedly. To accomplish this grand end I enter the arena, designing to consider and examine thoroughly the whole world or microcosm which the soul inhabits; for I think it is in vain to seek her anywhere but in her own kingdom.'

His belief that he was the first to approach the subject of psychology in the manner of 'the experimental philosophy' is probably justified, but lest this should be interpreted, in these days of minute specialization, as evidence of mental aberration, it must be pointed out that Swedenborg's quest

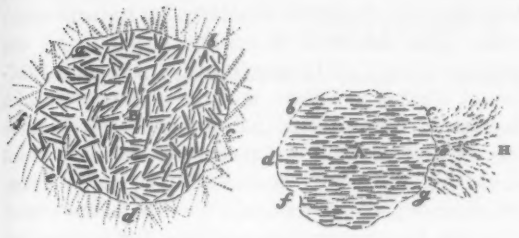


FIGURE 7 – *Swedenborg's diagrams of the 'vorticles' (our 'molecules') in (left) unmagnetized and (right) magnetized iron. This and figure 6 are from a modern edition of Swedenborg's* Principia.

was quite in keeping with the scientific spirit of the age. Readers of Newton's *Principia* will recall that he writes: 'This most beautiful system of the sun, planets, and comets, could only proceed from the counsel and dominion of an intelligent and powerful Being'. Further on he says, referring to this Being, 'to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy.' When Swedenborg set before himself the task of studying the soul through the human organism he was proposing a more modest inquiry than that adumbrated by Newton, and—though he was later to discourse of God also—a more narrowly scientific one. There is nothing at all here to indicate that Swedenborg was later to become anything but a typical scientist.

I am not qualified to appraise, or even to describe, Swedenborg's biological work. In his Presidential Address to the International Congress of Anatomists held at Heidelberg in 1903, Gustaf Retzius declared him to be the discoverer of the localization of the functions of the brain. Quoting from Max Neuburger, he stated that 'he leaped a whole century ahead of his age by the announcement of another discovery, for he was the first one to show that the cortical substance of the brain is the exclusive seat of the higher psychical activity.' He has also been credited with the discoveries that the body respires through the lungs, and of the osmotic permeability of living membranes.

The *Regnum animale* was never completed. Swedenborg's plans were changed by the reception of experiences of a mystical character which, though they did not immediately lead to the abandonment of his scientific work, ultimately turned his attention wholly to the contemplation of things outside the scope of the intellect. In 1743, when applying for further leave of absence to pursue his anatomical studies, he wrote:

'I am influenced interiorly by the desire and longing to produce during my life-time something real, which may be of use in the general scientific world and also to posterity, and in this way to be useful to and even to please my native country; and, if my wishes are realized, to obtain honour for it. . . . It is my own chief desire to bring this work to a close, and to return to my country, to my office, and to my property, where I shall, in tranquillity and ease, continue my larger work, the *Regnum minerale*, and thus be of actual use to the public at large in those matters which properly belong to the Royal Board.'

At this time, therefore, it is clear that he was contemplating an eventual return to his mining interests. But in the Preface to his *Arcana Cœlestia*, published in 1749, we read:

'It has been granted me, now for several years, to be constantly and uninterruptedly in company with spirits and angels, hearing them converse with each other, and conversing with them.'

The change he later described as a gradual one, beginning in 1743, when he was 55 years old. At any rate, almost simultaneously with the completed portions of *Regnum animale* in 1745 he published a book entitled *De cultu et amore Dei*, which, notwithstanding its title, belongs more properly to his scientific than to his religious productions. It deals with the Creation of the World, as described in the book of Genesis, which the author, like Newton and other religious scientists of those days, accepted as an account of actual historical happenings. In the *Arcana Cœlestia*, however, the same events are no longer regarded as facts but as allegories. From that time on, Swedenborg's interest in the rational interpretation of phenomena may be said to have disappeared. Natural events became significant only as symbols of eternal spiritual verities, and the remainder of his writings, which have secured him a far greater measure of recognition than his scientific achievements, were concerned with nature only in that aspect. Here, for our present purpose, we take leave of him. All that need be added is that in 1747 he resigned his position with the Royal Board of Mines, and thereafter lived mostly abroad, finally settling in London, where he died on 29th March 1772.

Considered on the purely intellectual level, there is no doubt that Swedenborg was a very remarkable man. His versatility manifests the breadth, as his original discoveries show the profundity, of his concern with natural phenomena. As would be expected, he did not allow the intensity of his search for knowledge to destroy his sense of the

importance of understanding what that search signified, as, alas, so many do now, He had a clear idea of the philosophy of science, and it is one that later developments have continued to justify.

'There are two ways', he wrote in 1740, 'by which to trace out those things in nature which lie either open before us, or are hidden from our eyes—viz. the *a priori*, which is also called the synthetical method, and the *a posteriori*, or the analytical method. Both are necessary in reflecting upon and tracing out one and the same thing: for in order to do so there is required both light *a priori*, and experience *a posteriori*. Now, while the learned among the ancients followed the former light as remotely and profoundly as they possibly could, those at a later period were induced not to accept anything as witness, unless it was confirmed by experience. Hence also some of the learned at the present day seem to have agreed to let thought rest, and to make experiments which would appeal to the senses; yet they did so with the hope and intent that some day experience would be connected with theory: for experience deprived of an insight into the nature of things is knowledge without learning, and a foundation without a building to rest upon it. The observations of the outward senses merely furnish data and give information about things which the understanding ought to investigate, and concerning which it ought to form its judgments.'

Why is it that such a gifted and successful investigator has left so small a mark on scientific history? The number of his discoveries that have had to be repeated later before being recognized as such is probably greater than that of any other man, and the lack of recognition was not because they were in any way inaccessible. How has such a remarkable thing happened?

I think it is because his eyes were fixed on a point too far ahead. He leaped to the goal by intuition, but was unable to explain the way in such terms that his contemporaries could tread it. To contribute to scientific progress you must see ahead of your fellows, but not too far ahead. If someone could have performed the Michelson-Morley experiment in the sixteenth century, Copernicus would have been discredited. If Newton had realized that his experiments did not testify that acceleration was absolute, we might have had no inverse square law of gravitation. If Darwin could have been convinced that acquired characters could not be inherited, we might not have had his theory of evolution by natural selection. The advance of science depends on the solution of each problem as it presents itself, regardless of what is to follow, and the discernment of a bee-line to the goal is useless to those who have feet but no wings. The piecemeal discovery of a devious but pursuable course is the way of progress. Swedenborg, like Browning's Grammarian, aimed too high for temporal success: his discoveries had all to be made again later, by other methods.

Science succeeded where medieval philosophy failed because it restricted its scope. Ceasing to ponder on God, on nature, and on human life, it timed the rolling of balls down slopes and scanned the rise of a liquid in a glass tube. By such a self-denying ordinance it discovered how trivialities worked, and left weightier matters to later generations, justified by the conviction that knowledge of the trivialities was necessary for the future task. Science may thus be said to have arisen from the realization of the impossibility of philosophy.

Swedenborg never attained to that realization. He accepted the science but could not acknowledge the impossibility.

'In respect to the soul and its various faculties,' he wrote early in the *Principia*, 'I do not think it possible that they can be explained or comprehended by any of the laws of motion known to us; such indeed is our present state of ignorance, that we know not whether the motions by which the soul operates on the organs of the body are such as to be reducible to any rule or law, either similar or dissimilar to those of our mechanics.'

Newton might have written that, but he did not allow the inapplicability of his laws to the organs of the body to prevent him from announcing them as universal laws of non-living matter. Swedenborg could not be satisfied with laws that did not cover the soul as well, and his aim was to derive completely universal laws. He failed, as any human being must have failed; but that, after all, is only half the story. He had his reward, though it was not a place of pre-eminence in the temple of scientific history.

NOTE. For various reasons it has not been possible for me in one or two cases to verify from the original sources the statements ascribed in this article to other writers. My information is taken from 'Swedenborg: Life and Teaching', by G. Trobridge (Swedenborg Society; fourth edition, 1935), which, so far as I have been able to check it, has been found completely trustworthy.

Science and technology in the Brussels Exhibition

The Brussels Exhibition, now in its third month, is the first of its kind to be held in Europe since before the war. One of the principal tasks of its organizers has been to make an inventory of the material wealth of humanity, and it is therefore not surprising, in view of the tremendous developments of the last twenty years, that science and technology receive more attention than on any previous occasion of a similar kind. The principal national pavilions show much scientific material, although they naturally vary considerably in the emphasis laid upon it, and in addition the International Hall of Science illustrates—irrespective of the country of origin—many of the great scientific discoveries of recent years. Before the Exhibition closes it will have been seen by some forty or fifty million visitors, and therefore it is, quite apart from other considerations, a major contribution to the very important task of interpreting science to the general public and of illustrating its relationship to everyday life.

On such occasions each participating nation seeks not only to display its achievements but to convey something of its way of life and modes of thought. While the highly artificial atmosphere and limitations of space of any exhibition inevitably prevent it being truly representative, the observant visitor to Brussels has a unique opportunity of comparing one nation with another and learning something of the complex world in which we live.

Such a conspectus can be of considerable practical importance, quite apart from being of great general interest. In this connection the Great Exhibition held in London in 1851, the ultimate ancestor of the present world fair in Brussels, deserves our consideration, for it is sufficiently a part of history for its significance to be clearly discerned. For Britain it was an event of great consequence, for it determined an important part of national policy for many years ahead. In 1851 the international jury awarded Britain prizes in almost all the hundred categories of manufacture. To the complacent, this was sufficient evidence of easy supremacy, but the more perceptive of British observers saw in the exhibition alarming evidence of strong competition from abroad. This observation had the important result of initiating in Britain a concerted effort for more and better technical education, the great importance of which had already been realized by most other European nations. The Paris Exhibition of 1867 showed how well justified had been the forebodings of the critics: then, only sixteen years later, Britain gained barely a dozen awards. This result greatly assisted those who were working for educational reform, and although the lack of success was disappointing the effect of the exhibition was salutary, and in the long run was much to the advantage of British industry. It is fair to say that its effects are still felt.

This is an old story and relates to a world very different from that now represented at Brussels. It is, however, worth recalling as a reminder that international exhibitions are not ephemeral affairs for the entertainment of the masses but can, if properly interpreted, usefully influence national policies for many years ahead. If the Brussels Exhibition has such an effect it will serve a most useful purpose. Some lessons are already clear. One, most forcefully conveyed, is that relatively backward nations who wholeheartedly foster the development of science and technology can in a remarkably short space of time challenge, and in some respects surpass, those who have been much longer in the field but have failed fully to appreciate the importance of this new branch of learning. Conversely, it shows that those which have neglected science and technology do so to their detriment, for they remain backward. It strikingly illustrates, too, that if science is properly used it can bring great benefits to mankind and is by no means the destructive force it is still too often supposed to be. The evidence of the Exhibition must greatly strengthen the arguments of those who, in many countries, are urging the need for the inclusion of more science in general education.

At the same time it must not be forgotten that the Brussels Exhibition is designed to display man's spiritual as well as his material wealth, and it is a useful reminder that technical progress alone is sterile. It is particularly pleasing to see how often exhibits of a technical nature have been satisfactorily united with others of a more general character, for it is of the first importance to show that science is a part of our modern culture and no longer stands in isolation.

Ocean waves

G. E. R. DEACON

The great practical and theoretical difficulty of studying ocean waves has resulted in their being much less intensively studied than most other forms of wave motion. During the past decade, however, important progress has been made, and this is here reviewed. It seems clear that further advance depends in the main upon more theoretical and experimental research on the basic physical processes. Such research promises results of interest not merely to scientists but to sailors, ship designers, and civil engineers concerned with coastal works.

INTRODUCTION

The great practical and theoretical importance of the regular oscillations that constitute wave motion needs no emphasis. The study of such diverse subjects as acoustics, optics, radio telegraphy, atomic structure, tides, and diseases of the heart, has demanded close investigation of wave forms, and it is surprising that the waves that have been by far the longest known, and give the name to the whole genus, have been studied in detail only comparatively recently.

The delay has, however, not been without good reason. Electromagnetic and acoustic waves are more easily studied than sea waves. Realistic patterns of wind and waves are not easily reproduced in the laboratory, and the difficulties of making observations at sea are formidable. In the absence of precise information about the nature of the phenomenon, theoretical workers had to be satisfied with the solution of rather formal problems. One textbook aptly describes the early theoretical work as mathematically beautiful rather than practically applicable. While a closer relationship between theory and observation would have been fruitful, progress had to wait for recent advances in hydrodynamics and electronics, particularly in the study of turbulent flow patterns and the theory of random signals.

Progress depended also upon a practical stimulus powerful enough to provide the considerable funds and the specialists required, which stimulus was given by the military needs of the last war. It was then necessary, especially for the great landings on the north coast of France, to improve methods of predicting the general pattern of waves from meteorological charts and forecasts and to learn something about the changes in offshore submarine bars. Subsequently interest has extended to problems of coastal engineering and the study of ships' motion. As the subject has become more manageable more scientists have been attracted, with the result that today steady progress is being made. The purpose of this article is to review what has already been achieved and to indicate the lines of research most likely to be fruitful in the future.

The greatest progress has been made within recent years, but it would be unjust to earlier workers, especially those of the nineteenth century, to ignore their contributions. A well-known feature of sea waves is that they are not of uniform amplitude. Traditionally, every seventh wave is an exceptionally high one, although Ovid gave this distinction to the tenth. In fact there is no such simple rule, but there is sufficient regularity in the alternation of high and low groups of waves as they approach a beach to suggest close comparison with the well-known beating of two sound waves. Beating presupposes a mixture of waves of different wavelengths: sometimes they get into step and reinforce each other, sometimes they are out of step and partly neutralize each other. To apply this conception to sea waves, however, involves theoretical and experimental difficulties that could not be faced until very recently. Although as long ago as 1876 G. G. Stokes compared high and low groups of sea waves with sound beats, his classical theoretical work and that of F. Gerstner, A. L. Cauchy, S. D. Poisson, Kelvin, Rayleigh, and others was based on the assumption that there was a single train of waves with a simple harmonic profile. In 1947 H. U. Sverdrup and W. H. Munk [1] acknowledged the interference of waves, but they and most others attempting wave prediction had still to assume that there is a dominant wavelength of larger or 'significant' waves which is recognizable and for which theoretical and empirical relationships can be developed.

Great waves are very impressive, and measurements of their height are of general interest. During six years of recording at a point a mile from the north coast of Cornwall the highest wave

was eleven metres, but fifteen metres is not uncommon in the open sea. Eighteen-metre waves have been recorded on one occasion (figure 3).

WAVE ANALYSIS

According to modern views, the sea surface can be considered as the sum of an infinite number of simple wave trains of different frequencies, with a continuous frequency spectrum. Local wind conditions and the travel of waves from distant storm areas determine how at any particular point the energy is distributed among the different wavelengths. Changes in the spectrum and of the energy contents of different wave bands can now be accurately estimated, the degree of accuracy depending on the length of the record and the rate at which the wave pattern is changing.

The first serious attempt to make a spectrum analysis of sea waves seems to have been made in 1944–45 by an oceanographical team at the Admiralty Research Laboratory, Teddington. They used records of wave height obtained from three recorders laid on the sea bed along a line running out to sea from the north coast of Cornwall; they were respectively 1000, 2000, and 3000 yards from the shore. Even though the recorders were comparatively so close together, it was found that the interference between waves of different wavelengths and different speeds was so great that waves and groups of waves identified on the record of one instrument could not be recognized on that from the next. This result confirmed observations of waves travelling in deep water where an individual wave crest cannot be followed very far with the eye before it loses its identity.

These Cornish experiments led to the conclusion that there could be little improvement in the prediction of wave patterns until the distribution of energy between the various wave-bands could be computed with the same precision as in other branches of physics.

The first sea-wave analyser was described by N. F. Barber, F. Ursell, J. Darbyshire, and M. J. Tucker [2] in 1946. A wave record in the form of a black profile on photographic paper 14 cm wide was stretched round the outside of a drum 76 cm in diameter. A strip of light fell transversely on the record, and the intensity of the scattered light was measured with a photocell. In this way an electrical reproduction of the wave profile was obtained when the drum was rotated. The electrical signal was fed into a fixed-frequency filter, covering a narrow band, the output of which was rectified and recorded on another paper strip. The drum was set spinning and its speed allowed slowly to decrease under the effect of friction. In this way each wavelength in the record came in turn into resonance with the filter and the recorder drew an amplitude spectrum. To calibrate in terms of wave period, a second record, containing known wave periods, was simultaneously analysed by another optical system and filter on the opposite side of the drum.

The important innovation in this instrument was the continuous decrease in the speed of rotation of the drum. With this apparatus a complete Fourier analysis took about fifteen minutes, care having to be taken to make the rate of change of frequency match the characteristics of the filter, so that full resonance was obtained for each harmonic. Attempts to speed up the process led to difficulties. In modern forms of the instrument (figure 2) the speed of the drum is controlled by reference to the exponential discharge of a capacitor through a high resistance. It has also been made considerably smaller so that it can be taken to sea; a brief account has been written [3].

Many alternative techniques have been advocated which can usefully supplement our simple procedure, but this has proved far too useful to be lightly given up. It gives the information in the form in which it is needed for the prediction of swell and gives more than can be obtained by simple autocorrelation. It has led to many advances, such as the formulation of a new theory of microseisms and an extension of the theory of ship movements from the treatment of model ships in regular waves to that of real ships in complex wave patterns.

Obtaining wave records for analysis is no longer a serious problem. Tucker [4] has described apparatus that has been used to measure waves at the coast, on the sea bottom a mile offshore, from aeroplanes, and from ships. A wave recording buoy is shown in figure 4. Measurement from ships is the most striking development of all. It allows measurements while moving [5] by an ingenious combination of pressure recorders and accelerometers. These measure the varying pressures at two points below water on opposite sides of the ship; coupled accelerometers make allowance for the vertical motion of the measuring points.

Such wave recorders have already been fitted in ten ships—three British, three French, and four American—and already a vast amount of material for analysis has been collected. D. E. Cartwright [6] has used these shipborne recorders to deduce the direction of travel of waves from the apparent

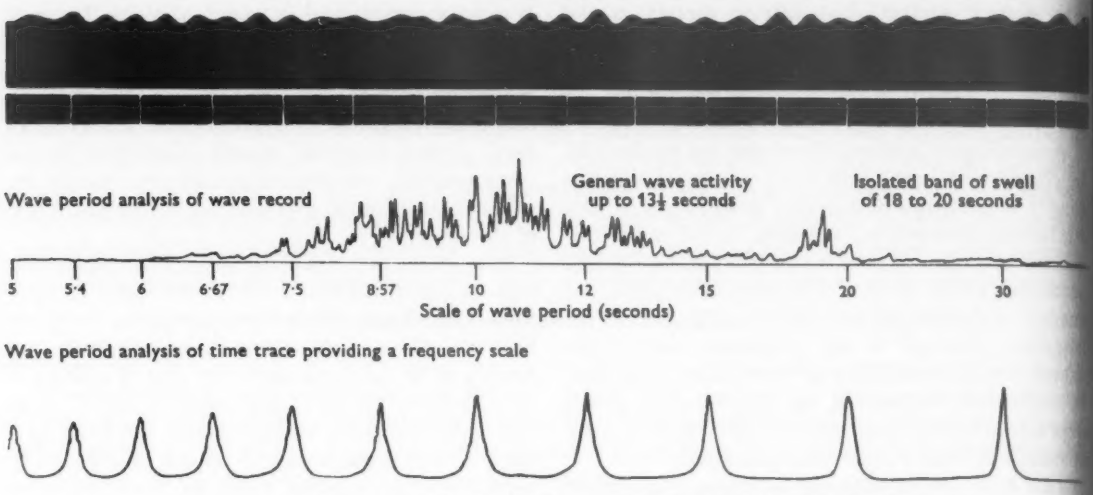


FIGURE 1 – *A wave record (top), with its wave period analysis immediately below.*

changes in frequency as the ship steams round a regular twelve-sided figure. Combining the results with simultaneous recordings of roll, pitch, and heave, he and L. J. Rydill [7] have made an important extension to the theory of ships' motion.

WAVE SPECTRA

Analysis of waves has proved very informative. Figure 1 shows part of a typical wave record and the corresponding wave spectrum. Most of the recorded wave energy is contained in waves with periods between seven and fourteen seconds; the remainder is in a separate band of longer waves with periods between eighteen and twenty seconds. The interpretation of such spectra in terms of prevailing meteorological conditions is often difficult, but important general conclusions can be drawn from the analyses of waves produced during comparatively simple meteorological conditions. Such waves behave as though they originate in a large number of point disturbances distributed throughout the area over which the wind blows. The maximum wavelength corresponds to the greatest wind strength; as will be seen, this correlation between wavelength and the strength of the causative wind is very important.

What happens at a time of active wave generation is not clear, but there must be some sort of interaction between various wave components resulting in energy being transferred from short to long waves as the dominant wavelength increases. After the storm is over, however, the interaction seems to cease and all the component wave trains seem to travel independently of one another. The result is a wave pattern which at any instant is the sum of a large number of wave trains having random phase relationships and covering more or less the same range of wavelengths. These wave trains spread out about 30° to 40° on either side of the predominant wind direction.

In figure 1 the spectrum begins with waves having a period of six seconds. This is because the movement in a wave decreases rapidly with depth, being reduced to about one-twentieth of the surface value at a depth of half a wavelength. This particular spectrum was made with a pressure recorder lying on the bed of the sea at a depth of about 25 metres, and the components of shorter wavelengths were not recorded. The general activity ends with waves having a period of fourteen seconds, because the causative wind, measured 30 feet above the surface, was about 28 knots. The waves with periods of eighteen to twenty seconds could be attributed to a distant storm in which the wind exceeded 40 knots.

Such a storm produces wave trains comprising a full range of waves with a periodicity up to twenty seconds. Those with periods less than eighteen seconds have not reached the recording station at the time of figure 1 because of their relatively slow speed of travel. As the disturbance spreads out from the storm centre the longer waves get farther and farther ahead of the shorter. The first swell to arrive on a distant shore has a period of about twenty seconds, and even after allowing for the increase in height due to shoaling it may have an amplitude of only a few centimetres and a wavelength of several hundred metres. It may not

be visible except over shallow ground, where it becomes slower and the crests become closer and steeper; it would then be called ground swell. Later parts of the record will reveal the slower, shorter swell. The highest swell will have a period of about fifteen seconds. After the arrival of this, the swell will gradually decrease in both amplitude and wavelength until the effect of the distant storm dies away in the shortest waves that can travel so far without being completely dissipated.

More precisely, the component wave trains, if not too steep, travel independently from the storm area with the group velocities appropriate to their periods. If the generating area is well defined and far enough from the recorder the successive wavelengths can be distinguished. The distance of the storm centre can be calculated from the difference in arrival time of longer and shorter waves. The implicit assumption that the component wave trains travel independently, with the velocities predicted by classical wave theory, is justified by the fact that the computation proves correct whenever it is made in meteorological circumstances which clearly provide only one storm centre.

Disturbances attributable to specific storms may travel immense distances. When there are no storms in the North Atlantic, wave recorders installed off the Cornish coast can be operated at their maximum sensitivity. The wave spectra then often show a band of swell the periodicity of which diminishes so slowly that the calculations based on relative velocities and difference in time of arrival leave no doubt that it is generated in storms near Cape Horn. It is usually only a few centimetres in height. Similarly, records made recently on the coast of California have demonstrated the arrival of swell generated south of New Zealand.

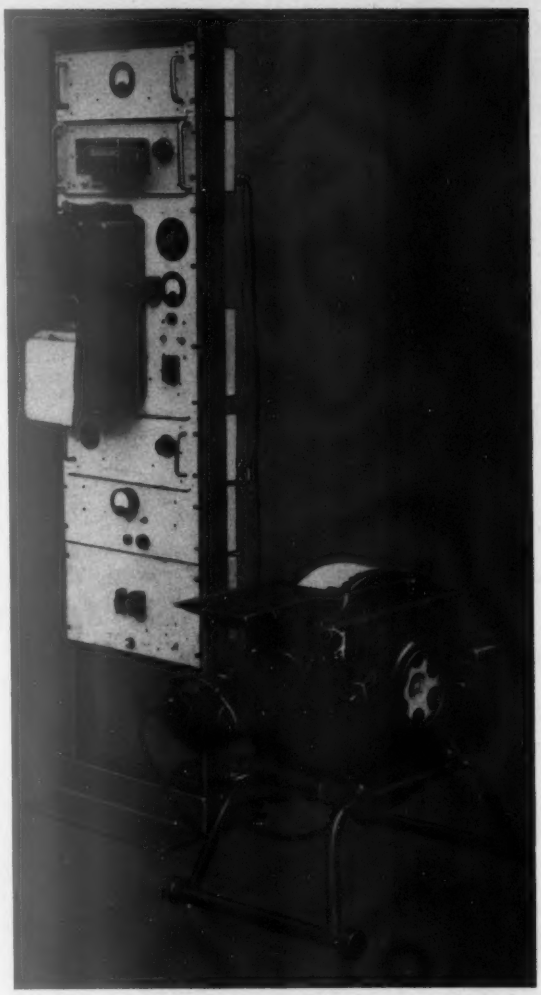


FIGURE 2 – *A wave analyser. The drum bearing the wave record can be seen on the right.*

METHODS OF WAVE PREDICTION

As has been shown, wave spectra can in appropriate circumstances be related to meteorological conditions. Conversely, it ought to be possible to predict the wave pattern at any particular point from the relevant meteorological data; waves derive their energy from the winds, so that if we have wind charts and forecasts we should be able to predict the waves. In fact, however, progress along these lines has not been quite up to expectation. The wave predictions made for the landings on the Normandy beaches during the last war were remarkably accurate, but greater difficulty has been experienced in forecasting for truly ocean coasts and for islands in mid-ocean. Here the newly acquired information about the rates of travel of different wave-bands has been useful. Although all the storm energy may be communicated to the waves in half a day, its arrival at a distant coast may be spread over several days. The spectral distribution also affects the rate of decay of the wave energy because shorter components lose energy more quickly.

A serious limitation to progress is our lack of knowledge of the physical processes by which the energy of the wind is communicated to water. It is believed that the main responsible factors are the development of positive pressures on the windward slopes of waves and some reduction of pressure on the leeward slopes, and the frictional drag of the wind over the waves; but we have little or

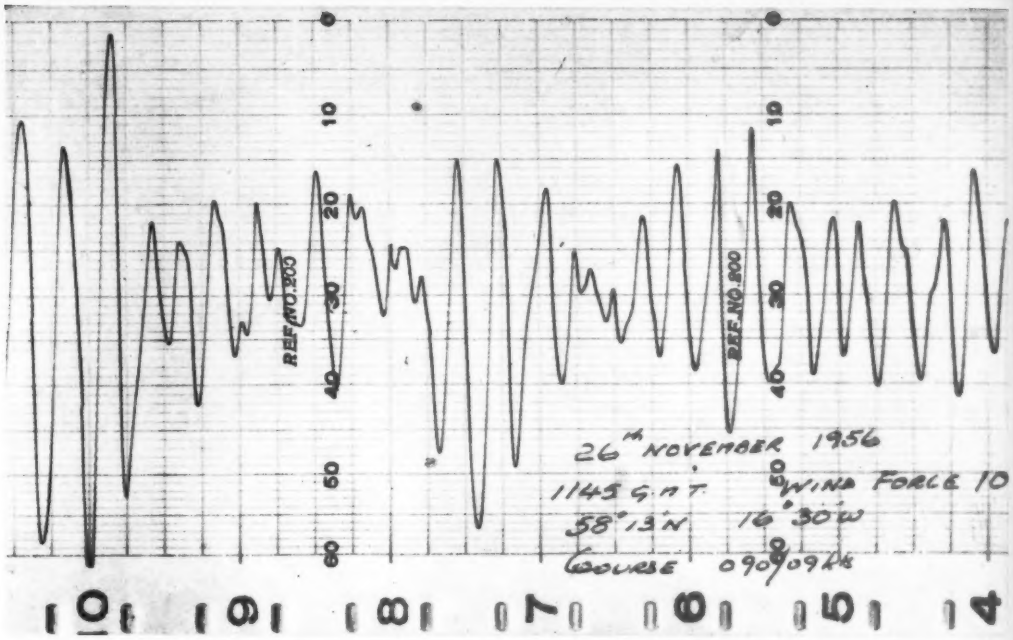


FIGURE 3 – *A record of some large waves, taken by the Ocean Weathership 'Weather Explorer', on 26th November 1956. The vertical scale on the record measures wave height in feet, and the high wave on the left is about 60 ft from crest to trough. The horizontal scale has four divisions to 1 minute.*

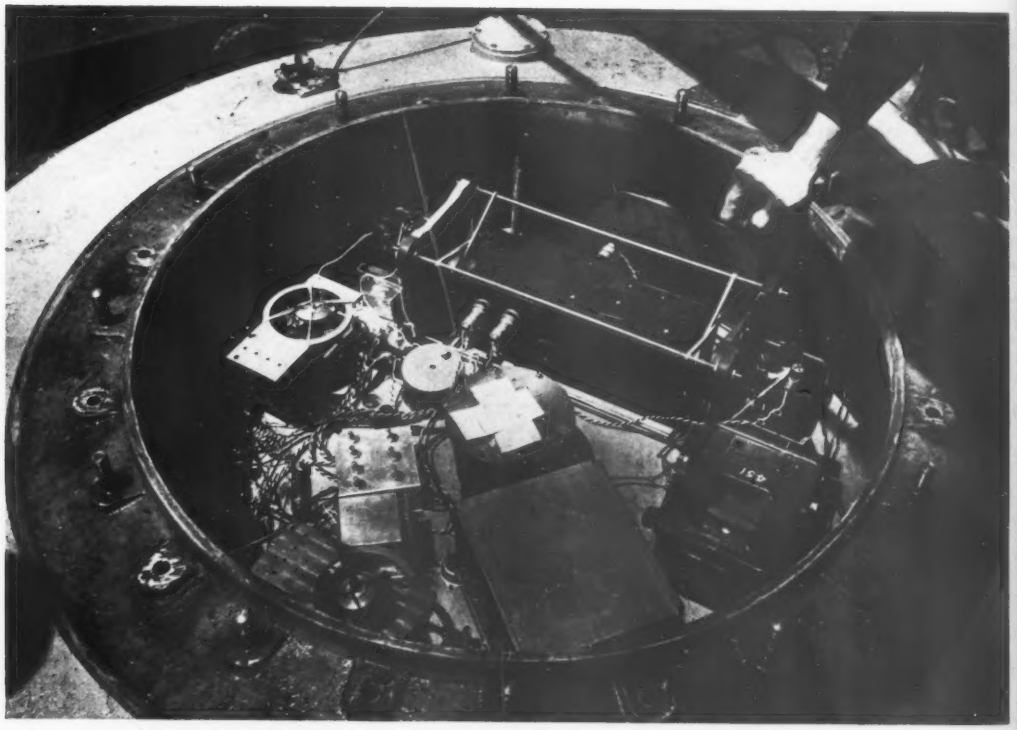


FIGURE 4 – *A wave-measuring buoy used to study variations in air pressure close to the sea surface in relation to the heights and slopes of the waves. The two gyroscopes on the left provide records of the slopes of the waves in the direction of the wind and at right angles to it; the round box in the centre contains a vertical accelerometer, and behind it is the recording camera. In the foreground is the electronic apparatus for doubly integrating the accelerometer output to give the wave profile and for recording the output of the microbarograph which is contained in the lid, which has been removed. Most of the batteries are hidden under the rim. Records were obtained in winds of up to 35 knots.*

no real information, though the necessary measurements are now being made. The gradual diminution in the height of waves as they leave the storm area may be due partly to air resistance, but the spread of waves as they move outward and the dissipation of energy by viscosity or turbulent decay seem to be more important.

The problem has been approached empirically through the analysis of meteorological data and wave records, but the results are disappointing, mainly because the observations are made on such a wide range of natural phenomena and are affected by variable depths, tides, and other factors. Real progress will probably be made by combined theoretical and empirical approaches. Work by O. M. Phillips [8], for example, gives a reasonable picture of the early stages of wave generation in terms of known physical quantities, e.g. the normal pressure fluctuations due to atmospheric turbulence.

SOME RECENT ADVANCES

The fact that we can now make reliable frequency analyses has stimulated studies of waves themselves, of ships' movement, and of microseismic ground oscillations. M. S. Longuet-Higgins [9] and J. Darbyshire [10] have shown that microseismic ground oscillations are caused by the interference of sea waves travelling in opposite directions behind rapidly moving storms, or by the interference of advancing waves and those reflected back from a coast. It seems likely that measurements of microseisms can be used to obtain useful information about distant storms and approaching swell even in the absence of meteorological charts for the region in question.

A picture of the sea surface as a two-dimensional random process has been suggested by W. J. Pierson in America and by M. S. Longuet-Higgins [11] in Britain, and they have worked out many of the statistical distributions connected with such surfaces. New techniques have been developed for measuring such properties as the intervals between crests in any direction, the number of points of reflected sunlight per unit area and the statistical distribution of their velocities, and the lengths and directions of wave contours. These will give us two-dimensional spectra and reveal much about the distribution of energy in the surface. C. Cox and W. H. Munk [12], for example, have made a statistical survey of the distribution of energy in the sea surface from photographs of sun glitter. Better knowledge of the nature of the surface should assist research on the mechanisms of the processes by which wind energy is converted into wave energy.

With growing knowledge have come increasingly important practical applications. Statistical work has contributed, for example, to the study of ships' motion. Cartwright and Rydill have been able to calculate, from the wave spectrum, the probabilities of the occurrence of maximum amplitudes, velocities, and accelerations of movements in ships' structures.

There have also been notable achievements in the study of beach currents due to waves. This has included water transport due to waves [13], movements of sand on the sea bottom [14], and the rip currents so dangerous to bathers [15].

In the latter connection F. P. Shepard found evidence of strong outward currents at various points on all coasts where there are large breakers. They usually consist of narrow streams that run out through the breakers and fan out beyond them. They can occur in places where there is little tidal movement and can be recognized from the air or from high cliffs as an outward movement of discoloured water or foam. If caught in a rip current, the swimmer can escape by swimming parallel to the shore.

ACKNOWLEDGMENT

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The origin of the solar system

SIR HAROLD SPENCER JONES

Astronomers have long been interested in theories of the origin of the solar system, and these theories can be divided into two groups. Theories of both groups, the one assuming that the system has been formed by the slow evolution from a primordial mass, and the other based on the idea of a catastrophic interaction of the Sun and another celestial body, are examined here, and the probable development of a satisfactory theory is suggested.

It has long been recognized that the solar system shows so many conspicuous regularities that it could not have been formed by chance. The planets revolve around the Sun and rotate around their axes in the same sense, which is that of the Sun's rotation. Their orbits, which are of small eccentricity, are nearly coplanar. Most of the satellites also move in orbits of small eccentricity that are inclined at small angles to the equator of the associated planet, and their motions are in the same sense as those of the planets. The outer satellites of Jupiter and Saturn and the satellites of Neptune are, however, exceptions. The distances of the planets from the Sun show some degree of regularity, represented approximately (except for Pluto, the most distant planet) by the empirical law known as Bode's law.

The planets can be divided into two groups: the four inner planets are of relatively small mass (the Earth being the most massive) and of high mean density (4·2–5·5 g/cm^3); the four major planets are much more massive and are of low mean density (0·7–1·6 g/cm^3). Pluto, the outermost planet, is of small mass, and it seems probable that it was originally a satellite of Neptune.

A remarkable feature of the solar system is that almost all its angular momentum is in the orbital motions of the major planets (98 per cent), while almost all its mass (99·87 per cent) is concentrated in the Sun.

During the past two centuries many theories have been proposed to account for the origin of the system. These theories can be divided into two groups. One group supposes that the system was formed by the slow evolution of a primordial mass; the other group is based on the assumption that it was formed as the result of interaction of a catastrophic nature between the Sun and another celestial body. The early hypotheses about the origin of the system illustrate these alternative views. Buffon in 1745 suggested that the planets were formed from fragments of matter torn off the Sun as a consequence of its having been struck by a huge comet. Kant in 1755 supposed that the Sun was originally at the centre of a nebula, which was in rotation round the Sun under its gravitational attraction. Mutual collisions between the separate particles caused the nebula to flatten out into a disk, within which a gradual condensation around the denser aggregations ensued. These aggregations, each in rotation, condensed further to give rise either to a planet or to a planet with a system of satellites. Laplace in 1796, starting from the same hypothesis as Kant, supposed that the nebula was originally hot and that as it cooled it gradually contracted, its rotation becoming more rapid because of the conservation of its angular momentum. When the centrifugal force at the equator became greater than the gravitational pull, a ring of matter split off round the equator. This process was repeated several times, each of the rings aggregating into a single mass; in some of these a similar process was supposed to have occurred, so giving rise to the satellite systems.

There are many objections to the hypotheses of Kant and Laplace. Among them is their failure to account for the distribution of angular momentum in the system. If, for instance, the whole of the angular momentum of the system were concentrated in the Sun, its period of rotation would be about 12 hours; but the centrifugal force at its equator would then be only about 5 per cent of the force of gravity, so that rings of matter could not possibly be thrown off as supposed by Laplace.

In order to meet the angular momentum difficulty, many attempts have been made since the beginning of this century to develop a satisfactory theory based on the action of an external body. Such action would involve changes in the distribution of the angular momentum, so that it

is conceivable that a mechanism by which the planets could be formed and endowed with their large angular momentum might thereby be provided. The earliest of these attempts was the planetesimal hypothesis developed by T. C. Chamberlin [1] and F. R. Moulton [2], who supposed that at some remote epoch another star passed very close to the Sun, causing a succession of huge eruptions to occur on it, each accompanied by the ejection of matter. The ejected matter was pulled round by the attraction of the passing star, more or less in the plane of the relative motion of the two bodies. As the matter cooled, it condensed into liquid drops which quickly solidified, forming the planetesimals. The denser portions of the matter are supposed to have formed the nuclei of the planets, which grew by accretion of the planetesimals as they circled the Sun. The residual matter formed a resisting medium, the effect of which was gradually to reduce the eccentricities of the orbits.

H. Jeffreys [3] pointed out that the frequency of collisions between the planetesimals, and their high relative velocities, would cause them to be volatilized long before accretion could have any important effect. He and J. H. Jeans [4] modified the theory by supposing that the passage of the star near the Sun drew out a long curved filament of matter, which was unstable and broke up into a number of separate fragments in orbital motion round the Sun. Some of these fragments, while still only partially condensed, were broken up on their first return to the vicinity of the Sun, so producing the satellites. Some of the matter that was drawn out fell back on the Sun, giving it a rotation in the same direction as the orbital motions. The theory in this form did not stand detailed examination. It could not, for instance, meet the angular momentum difficulty.

The hypothesis of a stellar encounter was modified in various ways in order to overcome these objections, thereby becoming increasingly improbable. Jeffreys [5] supposed that instead of a near approach of the star to the Sun there had been an actual collision. H. N. Russell [6], however, showed that on the most favourable assumption the angular momentum per unit mass of the matter from which the planets are supposed to have been formed would have been less than one-tenth of the average angular momentum per unit mass of the actual planetary system. To overcome this difficulty he suggested that the Sun might originally have been a binary star and that the intruding star collided, not with the Sun itself, but with its smaller companion. R. A. Lyttleton [7] investigated this problem and found that, under certain circumstances, both the colliding star and the Sun's companion could escape from the system leaving sufficient material to form the planets, while at the same time the angular momentum difficulty could be overcome.

The material drawn out from a star by a near approach or by an actual collision with another star would be at a very high temperature, so that the mean velocity of the atoms would be very large. L. Spitzer [8] showed that under these conditions the filament would expand under its own internal pressure much more rapidly than it would lose energy either by radiation or by turbulent convection. The filament would not condense into separate planets, but some of it would escape into interstellar space while the remainder would form an extended gaseous envelope around one or more of the stars involved. None of the modifications of the hypothesis of the close encounter or collision of the Sun and another star can meet this objection. All further attempts to account for the origin of the solar system have been based on the hypothesis that it has been produced by the gradual evolution of a primordial system. Such theories all start from the assumption that the Sun was at the centre of a diffuse cloud of gas and dust, which may have been an interstellar cloud that the Sun had entered or a cloud from which the Sun itself had condensed or was in the process of condensing.

A theory of the origin of the solar system has been proposed by H. Alfvén [9] based on the assumption that the Sun formerly possessed a large magnetic field and was in the last phase of its formation, with about its present temperature. The gaseous cloud surrounding the Sun was ionized by its radiation. The degree of ionization of any particular element would depend on the temperature and therefore on its distance from the Sun. In an inner region surrounding the Sun all the elements would be ionized, but, going outwards from the Sun, the progressively decreasing temperature would result in helium, hydrogen, oxygen, carbon, silicon, iron, and magnesium, among the principal constituents of the cloud, ceasing successively to be ionized.

The magnetic field around the Sun would tend, according to Alfvén, to impede the fall of the ionized constituents towards the Sun under its gravitational attraction, so that the chemical composition of the cloud would depend on its distance. Any given constituent, in its fall towards the Sun,

would reach a distance at which, because of its increasing temperature, ionization would set in. Any further fall would be stopped at a distance roughly proportional to the distance at which gravitational energy is equal to the ionization energy.

When any constituent became ionized, the electromagnetic forces would tend to accelerate it to the same angular velocity as the Sun. A magneto-hydrodynamic transfer of angular momentum from the Sun to the cloud would take place. The ionized gas would move along the magnetic lines of force and eventually be brought into the equatorial plane, where it served as the raw material for the formation of planets. Within a certain region around the Sun, gravitation would prevail, so that the material in that region would fall into the Sun. Alfvén gave reasons for supposing that four main clouds would be produced by this process, differing in composition and consisting respectively primarily of helium, hydrogen, carbon, and iron with silicon, though there would be no clear-cut separation between these clouds.

The theories of the processes by which planets and satellites are supposed to have been formed while the Sun was growing progressively by accretion were developed in great detail by Alfvén but need not be described here. The difficulty of this theory is that the magnetic field of the Sun, which was postulated as the agent that caused the material surrounding it to be suspended against gravity for long periods of time while the diffusive separation of different elements was taking place, is required to exceed a certain critical value. This critical value is 300 000 gauss, whereas the present magnetic field of the Sun is only about one gauss even at the surface. In addition, no adequate discussion was given of the mechanisms by which the magnetic field could suspend the cloud against gravity and by which the gravitational energy, as the cloud fell in, could be converted into thermal energy and energy of ionization.

C. F. von Weizsäcker [10] formulated a theory which was largely qualitative but which emphasized the importance of hydrodynamical considerations and of the effects of turbulence. He assumed that the Sun was at the centre of a local gas cloud, extending to about the present limits of the solar system and having a mass about one-tenth that of the Sun, which had condensed from an eddy in the general galactic gas cloud. Collisions between the various eddies in this cloud caused them to acquire angular momentum. The Reynolds number[1] for such a system is very high, of the order of 10^{11}, so that turbulence developed which, together with internal friction, changed the shape and orientations of the paths of the particles until they were eventually reduced to orbits of nearly circular shape in the vicinity of the equatorial plane of the Sun. Viscous forces came into play which, by slowing down the faster-moving inner parts and speeding up the slower-moving outer parts of the cloud, tended to equalize the angular velocities of the different parts of the cloud. There was thus a gradual transfer of angular momentum outwards, the material with low angular momentum becoming concentrated towards the centre, so that the slowly rotating central mass was surrounded by a faster-rotating flattened gaseous cloud.

The turbulent pattern was superposed on the Keplerian rotation. Particles of equal mean motion would collect into vortices and Weizsäcker assumed that the vortices would arrange themselves into a succession of rings, the most stable arrangement being obtained with an integral number of vortices in each ring. Between successive rings there would be steep velocity gradients, giving rise to large viscous stresses. Secondary eddies, in the nature of roller bearings, would form on the circles separating the main vortices, the rotation in these eddies being in the opposite direction to that in the vortices. Weizsäcker showed that condensation would start in these roller bearings and that in the later stages growth would occur by gravitational attraction when the mass exceeded a certain critical value. As there would never be perfect symmetry, the condensations would be of different sizes. The larger condensations would sooner or later attract the smaller, and eventually the eddies in each ring would come together and form a proto-planet. It was suggested that an analogous sequence of events would follow in the atmospheres of these planets, giving rise eventually to the satellite systems.

By assuming empirically that there were five vortices in each ring Weizsäcker was able to obtain a satisfactory representation of the relative distances of Mars, the asteroids, Jupiter, Saturn, and Uranus. S. Chandrasekhar and D. ter Haar [11], however, have shown that the

[1] This number, involving the velocity, density, linear dimensions, and viscosity of the fluid in motion, gives the conditions under which motions are similar when the qualities in question vary. In many cases flow which is laminar for low values of Reynolds number becomes turbulent for higher values.

eddies postulated by Weizsäcker could not arise from a turbulent medium.

The basic ideas of this theory have been used by G. P. Kuiper [12] to develop a modified theory, in which an important role is attributed to the process of gravitational instability. He supposes that the Kolmogoroff spectrum of turbulence[1] can be applied and concludes that large eddies may be found anywhere in the cloud and that small eddies may occur inside as well as outside the larger eddies. The eddies have comparatively short lives and are continually forming and dissolving. In order that condensations may take place it is essential that their density should exceed the critical value at which the internal attractions within the cloud are equal to the solar tidal force, for otherwise disruption by tidal action would occur. Kuiper supposes that the formation of planetary masses occurred essentially at the critical density for each of the condensations.

These condensations formed proto-planets, which at first were essentially of cosmic composition and which extended to about half the width of the ring from which they had drawn their material. Their masses must have been greatly in excess of the present masses of the planets, though their distances were approximately those of the present planetary distances. Kuiper considers that Bode's law merely represents the initial radial density distribution in the solar nebula.

In order to explain how the proto-planets subsequently lost most of their mass, with only the present planets surviving, Kuiper supposes that they were formed by contraction, before solar radiation was important, during the Helmholtz-Kelvin period, i.e. before the period of thermonuclear heating. Comparison between the roughly known energy-content of the proto-planets and of the present planets indicated that much energy had to be added to the proto-planets to make possible the evaporation of most of their material, and this energy could only have been provided by the Sun. He considers that after the Sun had completed its contraction and a rapid rise in its temperature ensued, the surrounding cloud became ionized and the angular velocity of the Sun was reduced by magnetic coupling to the ionized material in contact with it. Solar corpuscular radiation swept out the ions; the vacated spaces were replenished hydrodynamically from the denser regions and the new material was in turn ionized and swept away. Thus the entire interplanetary space was gradually cleared. Hot exospheres then formed in the outer fringes of the proto-planets, followed by evaporations to interplanetary space, the material so evaporated being continually swept clear. The evaporation of the proto-planet envelopes can plausibly account for the present planetary obliquities, except for that of Uranus.

The rotations of the proto-planets would initially synchronize with their orbital revolutions. As the proto-planet contracted, the rotation could no longer be synchronized and then became direct.

The five largest satellites (the four Galilean satellites of Jupiter and Titan of Saturn) were formed within the proto-planets by a process similar to that by which the planets were formed. The other satellites were of hybrid origin, starting with the collection of solid condensed matter before gravitational effects could become important. The subsequent loss of mass of the proto-planets led to a proportional expansion of the satellite systems. Some of the outer satellites could then have been lost to interplanetary space and subsequently either have been recaptured by the proto-planets to become irregular satellites or have become Trojan asteroids.

The outermost region of the solar nebula had a density below the limit at which proto-planets could be formed. Condensation products, such as water, ammonia, methane, etc., in the solid or frozen state must have formed in that region, because of the low temperature, and then have slowly collected to form larger aggregates. Kuiper suggests that these aggregates formed the comets, which were subsequently scattered through the system by the perturbing action of the planets.

The Soviet scientist, O. Y. Schmidt, has published a series of papers [13] on the origin of binary stars and of the solar system. He supposes that the Sun in its revolution round the centre of the galaxy passed through a cloud of interstellar matter and captured a great amount of it. Such capture is possible only if the velocity of the Sun relative to the cloud is small. In general this would not be so, and Schmidt infers that relatively few stars could capture clouds and develop planetary systems.

The cloud captured by the Sun consisted partly of gas and partly of dust particles and, because of its low density, must have had dimensions many times greater than the present solar system. The cloud contracted gradually under the gravitational attraction of the Sun. The particles of the

[1] According to Kolmogoroff, the eddy sizes in turbulent medium are distributed according to the law

$$F(\lambda)d\lambda = C\lambda^{-1}d\lambda$$

where λ denotes the eddy size.

cloud would have had the same general direction of motion around the Sun, but their individual orbits varied greatly in their size, in their elongation, and in their angle of inclination to the central plane of the cloud. Collision between the particles would eventually bring them into a flattened disk, consisting of dust and solid bodies, resembling Saturn's rings, revolving round the Sun and surrounded by a cloud of gas.

The larger particles increased in size through collection of smaller particles, and numerous aggregations capable of resisting the Sun's disruptive tidal force began to form inside the disk. The collisions resulted in decrease in the relative velocities of the particles and a consequent shrinkage of the aggregations, while a considerable part of the kinetic energy was transformed into heat and radiated into space. Those particles which, through collision, acquired very elongated orbits were captured by the Sun at their near approach, so that the Sun gradually acquired a slow rotation in the direction of the revolution of the cloud. The aggregations were considered by Schmidt to be the 'embryos' of the planets. If two embryos were moving along comparatively close orbits, the one nearer the Sun would grow primarily by acquiring particles with smaller specific angular momenta, while the one farther from the Sun would grow primarily by acquiring particles with larger specific angular momenta, so that their orbits would gradually shift towards and away from the Sun respectively. Schmidt showed that if a sufficiently large fraction of the original kinetic energy of the particles were transformed into heat, the planetary aggregation would acquire a direct rotation.

Schmidt gave reasons for concluding that, given a smooth distribution of matter in the initial cloud, the specific angular momenta of the planets should increase by equal steps outwards from the Sun. The specific angular momentum of a body revolving around a central mass is proportional to the square root of its orbital radius. Hence the square roots of the orbital radii of the planets should increase approximately in arithmetical progression. Such a relationship holds approximately for the inner planets, Mercury to Mars, and also for the outer planets, Jupiter to Pluto, the step from one planet to the next being much greater for the outer than for the inner planets. This is attributed to the difference in the properties of the inner and outer zones of the cloud. It is doubtful, however, whether these relationships have any real physical significance. Pluto, in particular, must for various reasons, be considered anomalous.

Schmidt's arguments are largely qualitative and it seems much more probable that the initial stage of planetary formation consisted of large protoplanets, which lost much of their initial material, as required by Kuiper's theory, rather than small embryos which grew by attraction and accretion of matter, as supposed by Schmidt.

The problem of formulating a satisfactory theory of the solar system cannot be regarded as yet solved, though important advances have been made in recent years. The theories that involved the close approach of another star to the Sun, or an actual collision have been abandoned and it seems reasonably certain that an explanation has to be sought in the evolution of an extended system containing gas and solid particles. Among the nearest stars about one half are known to be binary or multiple systems and the formation of a planetary system may be a special case of the formation of such systems. A fundamental difficulty is that the problem is not susceptible of exact mathematical analysis. The theory developed by Kuiper, though not entirely free from objection, appears at present to provide the most satisfactory explanation of the main features of the solar system and may with some modifications prove capable of leading to a theory that can be generally accepted.

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Parasite behaviour and the control of insect pests

GEORGE SALT

The main weapons at present in use in the attack on insect pests are the chemical insecticides. Biological control, which employs the natural enemies, predators or parasites, of the pest, is also of considerable importance and may have advantages in certain circumstances. It necessitates an extensive knowledge of the physiology and behaviour of these enemies: this article discusses relevant studies of the behaviour of some parasitic wasps.

The development and use of DDT, benzene hexachloride, and similar insecticides since 1945 has attracted so much attention to the chemical control of insect pests that other methods of control have been greatly neglected. There are signs, however, of reviving interest in other approaches to the problem. An increasing number of insect pests are developing resistance to insecticides. The cost of spraying rises as more concentrated poisons have to be applied. Some pests, for instance those widespread over great tracts of wild country and those that live inside plant tissues, are not easily accessible to chemical attack. For these and other reasons there is a place in the armoury of the economic entomologist for every means of combating pests, among them the method of biological control.

Natural enemies destroy large numbers of insects; the object of biological control is to enlist them to attack the particular insects that we call pests. If these allies are to play their full part in the control of insect pests they must be used with understanding. The efficient modern insecticides were developed from a vast programme of chemical research, and are used with detailed knowledge of their chemical properties. It is naive to suppose that natural enemies can be effectively used for control with inadequate knowledge of their biological nature, especially their physiology and behaviour. The purpose of this article is to consider some of the problems of behaviour that are met in using certain parasites, the ichneumons and related groups of parasitic wasps, for the control of insect pests.

When an ichneumon is ready to lay eggs it flies to a suitable habitat and there searches for a host. If we watch a female *Apanteles glomeratus*, a typical ichneumon, seeking larvae of the cabbage butterfly, *Pieris brassicae*, on young plants of brussels sprouts in our garden, we shall see it fly from leaf to leaf and walk briskly on their under surfaces. When it finds a young larva, it makes a careful examination and, if satisfied, pushes its ovipositor through the skin of the caterpillar and rapidly lays a number of eggs in the body cavity. Then it withdraws and prepares to attack another host. If all goes well, the eggs will develop, destroy the caterpillar, and provide a new generation of parasites to continue the good work.

Although the many details of behaviour involved in finding, choosing, and parasitizing a host are usually performed correctly, *glomeratus* often makes mistakes both of omission and commission. This parasite appears to seek larvae of *Pieris brassicae* only on cabbages, sprouts, and other cultivated brassicas; it fails to find them when they are feeding on certain other food plants, such as capers, *Tropaeolum*, and *Cakile maritima*, the sea rocket. Therefore, although almost all of the caterpillars on cabbages may be destroyed, the pest remains untouched on these other food plants, and survives to breed an abundant population in the next generation [1]. Sometimes, on the other hand, the parasite lays some of its limited number of eggs in larvae that are useless for the purpose, such species as *Pieris rapae* and *Mamestra brassicae*, which it comes across while seeking its proper host on garden brassicas. Long ago, N. F. Meyer [2] showed that the eggs the parasite mistakenly lays in *P. rapae* are often wasted, for even in caterpillars so closely related to the proper host many of the parasite eggs are unable to develop, and that part of the parasite egg-population is lost.

The behaviour of a parasite while choosing its host can easily be observed in the laboratory by the use of *Trichogramma* (figure 1), a ubiquitous chalcid-wasp, which does immense but inconspicuous good by destroying large numbers of pests while they are still in the egg stage. *Trichogramma*

is useful all over the world against a wide variety of insect pests: against the sugar-cane moth-borer in the West Indies, the cotton bollworm in Rhodesia, the oriental fruit-moth in Canada, and so on. There can be few parts of the world where it does not occur. For several years I have made a practice of collecting a few egg-masses of the cabbage moth, *Mamestra brassicae*, from the plants in my garden. After a few days indoors some, but by no means all, hatch, for every year from many of them, sometimes more than half, there emerge the tiny adults of *Trichogramma*. These run about in the corked vial, and if a fresh egg-mass of *Mamestra* or other similar moth is put with them, they soon settle down to lay their eggs in the new hosts. The behaviour of this small creature has been studied in detail and turns out to have some aspects which decrease its value as a controlling agent and others which make it more efficient than one might expect.

On the one hand, when choosing hosts in which to lay its eggs, *Trichogramma* makes what seem to us most absurd mistakes [3]. It will drill into empty eggshells from which the larval pest has already hatched. It will examine and try to drill into various small objects of about the same size as its usual hosts—such things as plant seeds, globules of glass, or even grains of sand—when these are pasted on cardboard and put before it like so many moth eggs on leaves. A most amusing experiment to watch was one devised to discover the limits of size of objects *Trichogramma* would accept as hosts. A globule of mercury smaller than a host egg was put into a petri dish with a female parasite, but attracted no attention. Minute droplets of mercury were run into the globule until it reached a size the parasite would accept. The female would then climb upon it, examine and accept it, and attempt to push in her ovipositor. The surface tension of the mercury was too great, however, and the little parasite would sit, sometimes for hours on end, gently bouncing up and down in her attempts to drill into this strange host (figure 2). While this was going on, other droplets of mercury could be added to the globule, which eventually reached such a size that the parasite no longer recognized it as a host, and moved off. T. E. Holloway [4] and P. Marchal [5] have observed *Trichogramma* attacking and actually laying eggs in small droplets of a gummy substance exuded from leaves of *Hibiscus*. This misguided behaviour must detract from the value of the parasite as a controlling agent by wasting part of its short adult life and some of its limited number of eggs.

In another respect the behaviour of *Trichogramma* is unexpectedly efficient. Let us consider a problem of the interaction of parasite and host populations, the sort of problem always involved in projects of biological control. Let us suppose there are 100 individuals of a pest in a unit of space, and that the available parasites attacking them have 100 eggs to distribute in that space. How many of the hosts will be destroyed?

We can calculate that, if the parasites distributed their eggs one at a time at random, on the average about 37 hosts will receive one egg; 18·5 will receive two eggs, 6 three, and 1·5 four. This inefficient distribution of the eggs would allow 37 pests to escape unparasitized. If, on the other hand, the parasites could distribute their 100 eggs with complete efficiency, all of the 100 pests could be destroyed. For this second situation to obtain, the parasites would have to be able to distinguish hosts already parasitized and avoid laying in them, so conserving all their eggs for the remaining healthy pests. What can we learn about the behaviour of *Trichogramma* in this respect?

The first experimental evidence [6] was obtained by a simple arrangement of hosts. A piece of graph paper ruled in tenths of an inch was cut in a circle to fit into the lid of a petri dish. Twenty-five healthy moth eggs were pasted within a quarter of an inch square, and the five rows and columns were numbered to facilitate reference to each egg (figure 3). The eggs used were those of a grain moth, *Sitotroga*, which were chosen because they are just sufficient in size for the development of one parasite. A female *Trichogramma* was put into the inverted petri dish, and a record was kept of every contact between the parasite and a host. It was soon apparent that discrimination between healthy and parasitized hosts was taking place. When the parasite touched a healthy host, it climbed upon it, examined it with its antennae for about 30 seconds, drilled through the eggshell, and about three minutes from the first contact went off to seek another. When, however, the parasite touched a host it had already parasitized, the examination lasted only five or seven seconds, and the parasite went off without further ado.

In each experiment the parasite was watched until it had made 25 attacks. Throughout the first part of an experiment the discrimination of the parasite appeared to be perfect. Towards the end, when most of the hosts had already been attacked and the parasite had run about for a long while without finding one of the few remaining healthy hosts, its urge to lay seemed to break down it

FIGURE 1 – *The egg-parasite* Trichogramma. (× about 40)

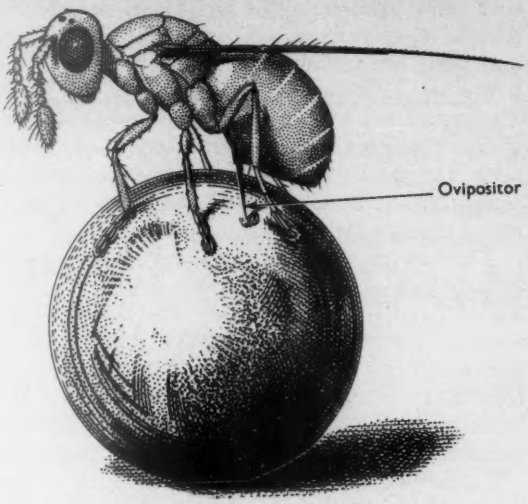

FIGURE 2 – Trichogramma *attempting to drive its ovipositor into a globule of mercury.* (× about 64)

restraint, and occasionally it accepted and drilled into a host already parasitized. Even in these cases, however, the parasite frequently withdrew its ovipositor as soon as it had penetrated the egg-shell and went off without laying a second egg. This could be shown by subsequent examination of the host. In five such experiments, each parasite being watched until it had distributed 25 attacks among 25 hosts, an average of 3 mistakes were made, most of them after twenty hosts had already been parasitized. The conclusion is inescapable that the parasite was able to distinguish between healthy and parasitized hosts, and to restrain itself from laying in the latter. Nevertheless, when most of the hosts were parasitized its restraint eventually broke down, and it occasionally attacked hosts already parasitized.

One immediately wants to know how the parasite was able to distinguish between healthy and parasitized hosts. The fact that hosts already parasitized are rejected after a cursory examination lasting only a few seconds shows that there is some external indication. In order to discover its nature, twelve eggs of *Sitotroga* were pasted on alternate squares of a graph-paper arena, as shown in figure 4*a*. A female *Trichogramma* was allowed to examine each of these hosts in the manner that normally precedes oviposition, but just as it accepted one and was about to unsheathe its ovipositor it was struck aside with a fine brush. These 12 hosts had been examined but not parasitized; let us call them 'dirty' hosts. The parasite was then removed and 13 clean, healthy hosts were pasted in the intervening squares of the graph paper so that a group of 25 hosts was formed as in the previous experiment (figure 4*b*).

A second parasite was then put into the dish and was kept under observation until it had made 25 attacks. In the few cases when the parasite accepted a 'dirty' host, it was pushed aside when it unsheathed its ovipositor, so that the host remained unparasitized. When it accepted a 'clean' host it was allowed to lay its egg, but while it was drilling into the next clean host it met, the one previously parasitized was removed and another clean egg was put in its place. The substitution

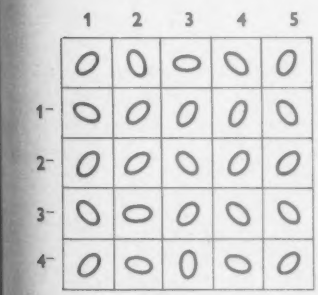

FIGURE 3 – *Arrangement of hosts on* ½ *in. graph paper for the first experiment.*

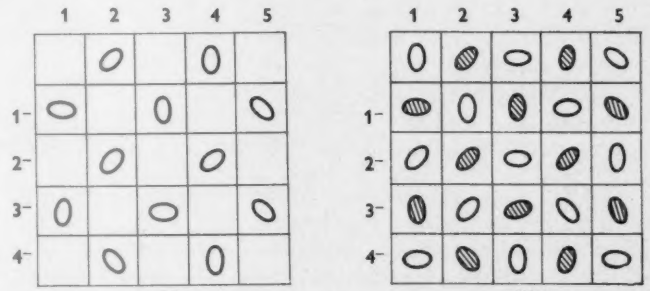

FIGURE 4 – *Arrangement of hosts:* (a) *in first phase of experiment,* (b) *in second phase. 'Dirty' hosts are shaded.*

was easily made, for the process of oviposition allowed 2½ minutes in which to make the change, and while the parasite was engaged in its attack it was not disturbed by movements of the brush placing a fresh host even in an adjacent square. This method maintained the situation that every time the parasite moved off a host it had just parasitized, it found 12 'dirty' and 12 'clean' hosts about it ready for its next choice.

The behaviour of the parasites used in eight experiments of this sort was very consistent. The 'dirty' hosts were approached and touched but were in nearly every case immediately rejected; the clean hosts were accepted. The conclusion is clear that *Trichogramma* was able to distinguish the hosts that had been merely walked upon by another parasite, and that it avoided attacking them [7].

Further experiments showed something of the nature of the trace that is perceived by the parasite. When 'dirty' hosts were washed with water on a brush and then offered to a parasite, they were accepted as readily as clean hosts. The trace is therefore water-soluble. When a large number of parasites were allowed to walk about in a vial and the glass on which they had walked was washed with a drop of water, the solution so obtained could be used to paint 'clean' healthy hosts, which were then rejected. From all this it appears that, in the act of walking on a host during the examination that precedes attack, the *Trichogramma* female leaves a chemical trace which is perceived by a parasite that comes along later, much as a dog can follow a scent.

Under natural conditions, rain might wash off such an external trace and cause mistakes. It is therefore interesting to find that *Trichogramma* has a safeguard in the form of a second discriminatory sense. When a parasite was allowed actually to parasitize a number of *Sitotroga* eggs and those eggs were then washed and offered to another parasite, they were examined and accepted as if they were healthy. But as soon as the ovipositor had pierced the eggshell and entered the host, the parasite usually pulled it out and ran off at once, without laying a second egg. Sense organs on the ovipositor appeared to warn the parasite that all was not well. Referring back to the first experiment, it was presumably this second sense that inhibited the laying of another egg when the parasites made mistakes and attacked hosts already parasitized.

This account has been restricted to *Trichogramma* because the analysis of behaviour has been carried further with it than with other parasites, but similar powers of discrimination have been demonstrated in several other forms. D. C. Lloyd [8] has shown that two ichneumonids, *Diadromus* and *Angitia*, and a braconid, *Apanteles plutellae*, avoid laying in hosts already attacked by parasites of their own or other species. F. J. Simmonds [9] has studied the similar behaviour of a chalcid, *Spalangia*. Dissection of pests collected in the field has shown that the distribution of several parasites in them is incompatible with a hypothesis of random distribution [6].

The intrusion of this matter of behaviour into our problem of the interaction of parasite and pest populations is thoroughly practical. When the parasites of our numerical example discriminate between healthy and parasitized hosts we can expect to have a more efficient distribution of their progeny than the 63 per cent parasitism that would result from random distribution. But because the restraint of the parasite eventually breaks down we cannot expect to find 100 per cent parasitism. What proportion of the pests will actually be destroyed depends on many factors, but especially on details of the behaviour of the parasites in finding and choosing their hosts.

The conclusion is clear that the behaviour of insect parasites plays a great part in fixing their value as pest controls; and it follows that we must know their behaviour before we can use them efficiently. Therein lies a difficulty, for the necessary investigations have not been done. The proportion of papers on chemical control and on biological control published in the journals of economic entomology since the war suggests that attempts deliberately to use parasites and predators are almost completely out of fashion. Perhaps in the future biologists will be asked to produce parasites to specification for the destruction of particular pests, as chemists develop their synthetic molecules. But a great deal of research, especially on the behaviour of parasites, will have to be done first.

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Ion exchange in clay minerals

C. B. AMPHLETT

Ion exchange is nowadays thought of mainly in terms of organic exchangers. It was, however, first studied in connection with what have since been recognized as ion exchangers in soil more than a century ago, before the concept of the existence of ions in solution had been suggested. Exchanges in clay minerals have recently become of interest in connection with the disposal of radioactive waste. The results obtained are of fundamental significance, and they, and the methods used, are described here.

Ion-exchange properties are exhibited by certain materials which are insoluble in water but are capable of exchanging cations or anions present in their structure for cations or anions dissolved in the external solution; the ion of charge opposite to that being exchanged plays little part in the reaction. There are two well-defined classes of ion exchanger, as well as a variety of miscellaneous materials. One class consists of the organic exchangers, in which the exchangeable ions are bound electrostatically to acidic or basic groups forming part of a large, insoluble organic counter-ion. The other class, the crystalline mineral exchangers, are those in which the exchangeable ions are present in a well-characterized crystal structure which permits exchange between ions of suitable size and charge. In all cases the exchanger as a whole is highly insoluble, but the exchangeable ions are mobile provided that charge neutrality is maintained.

Most present-day studies on ion exchange are devoted to organic exchangers, either the synthetic ion-exchange resins or materials derived by the treatment of natural products such as wood, coal, peat, and others. A flourishing industry has been built around the uses found for these materials, and new applications are constantly being described. Yet long before either these materials or their predecessors, the zeolitic exchangers, were first studied, the foundations of what are now known to be ion-exchange equilibria were investigated in studies of the retention of bases by soils. This plays a vital role in soil fertility and plant cultivation, and has continued to attract attention [1]; in recent years there has been an increased interest in this apsect of ion exchange.

Although it has long been known that the soil possesses purifying properties towards coloured solutions passing through it, the first systematic studies were carried out just over a century ago [2]. During the period 1845–50 experiments were made on the absorption by the soil of ammonia liberated by the passage of water over manure heaps, and the following salient facts were established. Ammonia is absorbed with the release of an equivalent quantity of calcium from the soil; potassium, sodium, and magnesium are similarly absorbed, although to differing extents, and in all cases the uptake is greater at higher concentrations. The absorption power of the soil, which was traced to the clay minerals present, is reduced by prior heating. The property of 'base exchange' is immediately familiar to us nowadays, but it is interesting to note that these typical ion-exchange properties were described and correctly interpreted many years before the concept of ions in solution had been suggested.

Before discussing the subject in more detail, we may note some of the consequences of ion exchange in clays and soils.

1. Cations supplied as fertilizer are retained in the soil against leaching by ground water, and thus remain available to plants over long periods, e.g. when K^+ is applied as kainite ($KCl.MgSO_4.3H_2O$) it displaces calcium from the soil and remains in the plant environment. The naturally occurring cationic plant foods, such as potassium, which are liberated by the weathering of primary rocks (e.g. the feldspars), are retained in the soil by ion exchange on the secondary minerals which result from the weathering.

2. The physical properties of a soil depend to a large extent upon the cations associated with the clay fraction, and may be changed by suitable treatment. Thus, sodium-rich soils are generally unsuitable for agricultural purposes because of their high water capacity, coupled with the excessive swelling tendencies of sodium clays in solution. Calcium-rich soils, on the other hand, give a better crumb-formation and provide a much better tilth. This may be achieved by heavy liming, in which Ca^{2+} ion displaces Na^+ ion to

give the desired properties. Much of the damage to agricultural soils caused by excessive sea-water flooding is due to the replacement of Ca^{2+} ions by Na^{+} ions, and the process may be reversed by heavy liming or by treatment with gypsum, which, being less soluble than lime, releases its calcium slowly over a longer period.

3. The plastic properties of clays also depend upon the cation present, sodium clays generally being more plastic than the naturally more abundant calcium ones. For brickmaking purposes the properties of clay may usually be improved by mixing soda ash with the clay before firing. In a similar way the civil engineer sometimes improves the qualities of soil foundations by altering the cation composition.

ION EXCHANGE IN CLAYS [3]

One of the principal sources of the property of ion-exchange displayed by soils is the clay fraction, and it is important to understand at least in outline how this property arises, since the ion-exchange capacities of clay minerals and soils may vary over a wide range (Table 1). The clay minerals form a

TABLE I

Cation exchange capacity of some clay minerals [3]

Material	Capacity, milliequivalents/100 g
Kaolinite	3–15
Illite	10–40
Montmorillonite ..	80–150
Vermiculite ..	100–150[1]

[1] Commercial vermiculite usually has a lower capacity than this because of the presence of hydrobiotite, which does not exchange.

very complex group of structures, many of which shade almost imperceptibly into each other through a variety of intermediate types. Only three types will be considered here, for purposes of simplicity. Basically, the clay minerals are built up from layer-lattice structures stacked parallel to each other in sandwich fashion, normal to the c-axis of the crystal. The different types of structure arise by variations in the pattern of stacking and by variations within the layers themselves.

Figure 1 shows in a very simplified fashion the way in which the structures of these three types of clay mineral are built up from units composed of tetrahedral $Si(OH)_4$ and octahedral $Al(OH)_6$ layers. In kaolinite, whose structural formula approximates to $Si_4Al_4O_{10}(OH)_8$, there is very

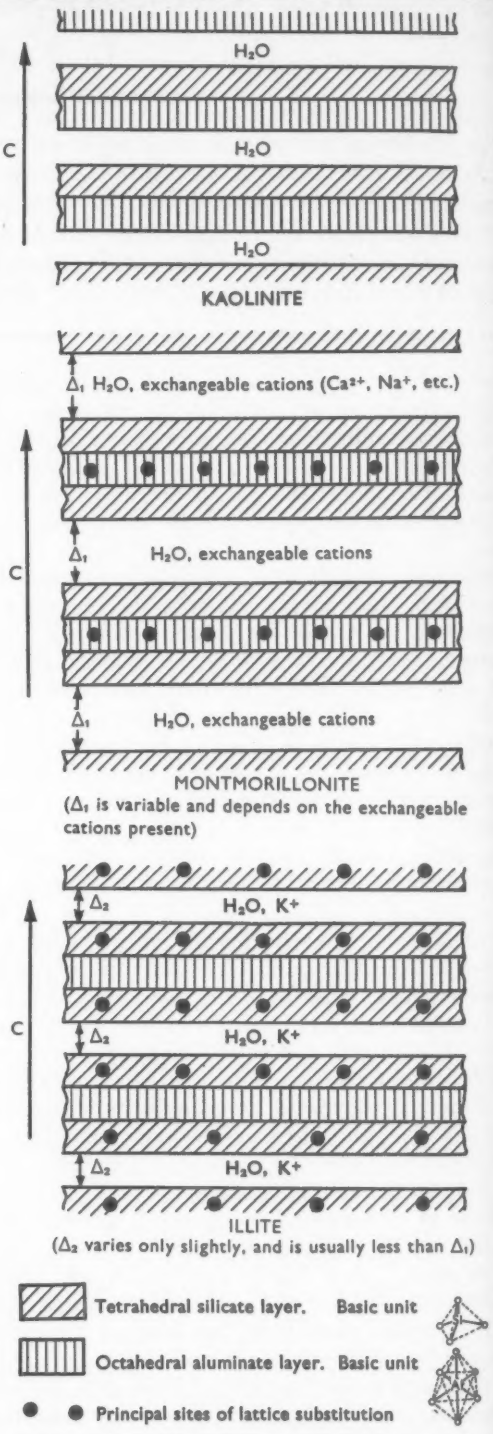

FIGURE 1 – *Diagrammatic representation of three important layer-lattice structures characteristic of certain clay minerals. Arrow indicates c-axis.*

little replacement of Si or of Al by other ions within the layers, and consequently no other ions are necessary in the structure to balance any charge variations. Forces between the unit layers are very weak along the c-axis, and the structural formula is simple. Montmorillonite consists of triple-layer units, with the aluminate layer sandwiched between two silicate layers, and the idealized structural formula $Si_8Al_4O_{20}(OH)_4.nH_2O$. In practice a range of minerals of this type exists, in which there is substitution within the octahedral layer (of Mg^{2+}, Fe^{2+}, etc., for Al^{3+}) and to a much less extent within the silicate layer also (Al^{3+} for Si^{4+}). The net negative charge thus set up is balanced in part by the entry of cations into the interlayer spacings along the c-axis. The units are thus bound by electrostatic forces, and although the binding is stronger than in the case of kaolinite it is not sufficiently strong to prevent swelling when the material is immersed in water; cation exchange then takes place between the ions present between the layers in the crystal lattice and those entering in the aqueous solution. Montmorillonite swells considerably in aqueous solutions, the value of Δ_1 (figure 1) varying with the water content and with the degree of hydration of the cation. The extent of substitution may be judged from the statistical structural formula of a typical dioctahedral montmorillonite ($Si_8(Al_{3\cdot34}Mg_{0\cdot66})O_{20}(OH)_4.nH_2O$ + exchangeable cations); it will be noted that the sum of the numbers of atoms of aluminium and magnesium is four.

The micaceous clay minerals (illite, muscovite) have basically the same structure as the montmorillonite group, except that substitution is almost entirely in the tetrahedral layer (Al^{3+} for Si^{4+}), and the balancing cation is largely potassium. Interlayer forces are much stronger than in the montmorillonites because of the shorter distances involved; consequently swelling is limited and exchange is more difficult. Muscovite has the structural formula $K_2(Si_6Al_2)Al_4O_{20}(OH)_4$; this is basically similar to that for montmorillonite, except that some silicon has been replaced by aluminium in the tetrahedral layer, with the introduction of an equivalent number of potassium ions to balance the negative charges produced.

Cation exchange may result from either of two mechanisms, viz.:

1. *'Edge and corner' exchange.* At the edges and corners of the layers there may be broken bonds or hydroxyl groups which represent positions available for the substitution of cations; the capacity will generally be low. All clay minerals display this behaviour: for some, e.g. kaolinite, it is the sole source of cation exchange.

2. *Interlayer exchange.* In this case the increased net negative charge due to substitution in the lattice is balanced by the entry of other cations into the interlayer spaces; the extent to which this is possible depends upon the size of the entering cation and the strength of the bonding between the layers. If bonding is strong, replacement of interlayer cations by exchange is difficult, and in such cases the resulting capacity may only be comparable with that due to edges and corners, as for example in illite. In clays having weaker interlayer bonding, e.g. montmorillonite, exchange is facilitated by the ease with which water enters between the layers on immersion in a solution, so that ions are free to move and exchange within the interlayer spaces. Such clays, in contrast to the other types, swell markedly in water and aqueous solutions, and X-ray diffraction patterns show a characteristic increase in the c-axis spacing between the layers, particularly when large, highly hydrated ions such as sodium enter the structure. In clays of this type the bulk of the cation exchange capacity arises in this way.

The different sources of cation exchange in clays affect not only the capacity, but also the rates and equilibria in different clays. Rates of exchange are generally greatest for clays such as kaolinite, where only the 'superficial surface' is involved, and least on the non-expanding interlayer exchangers, where appreciable time is required to penetrate into the pores and interlayer spaces. Similarly, the affinity series for the latter will depend much more upon the size of the ion and upon the pore size of the mineral, and some ions may be excluded on steric grounds. Furthermore, while particle size has little effect upon the capacity of montmorillonite, where superficial surface exchange plays only a minor role, the capacity of kaolinite is increased fivefold on reduction of the particle size from the 10–20 μ range to that of 0·1–0·05 μ; non-expanding interlayer exchangers occupy an intermediate position, as would be expected.

Although ion exchange reactions on clays are in general reversible, there are two important exceptions to this behaviour, viz. hydrogen exchange and cation fixation. Although clays may be titrated with either acid or alkali to give typical *p*H curves, it is impossible to replace all of the exchangeable cations by H^+ ion without partially destroying the structure by leaching out Al^{3+} ion from the layers into the interlayer spaces, where

it blocks exchange positions and leads to a reduction in capacity. Hydrogen ion exchange is thus not completely reversible, and this behaviour is most marked in the expanding interlayer exchangers. With sufficiently high acid concentrations the clay breaks down completely with solution of alumina and precipitation of silica. Physical destruction may also result from too high a concentration of Na^+ ions, owing to the high degree of swelling; deflocculation results, and the clay forms a colloidal suspension or solution. Sodium clays are generally difficult to dry and are in many cases thixotropic; this property is utilized in the employment of certain clays (e.g. Wyoming bentonite, a sodium montmorillonite) as a drilling 'mud'.

In cation fixation, a fraction of the exchange is found to be irreversible for certain ions and certain clay minerals; a notable example of this behaviour is the 'fixation' of potassium by certain soils. Although the cause is not completely understood, it is probable that certain cations of suitable size can migrate from the interlayer spaces into positions within the lattice itself, where the forces acting upon them will be much stronger. Other well-established cases include K^+ ion on illite, and the ions K^+, NH_4^+, Rb^+, and Cs^+ on vermiculite [4].

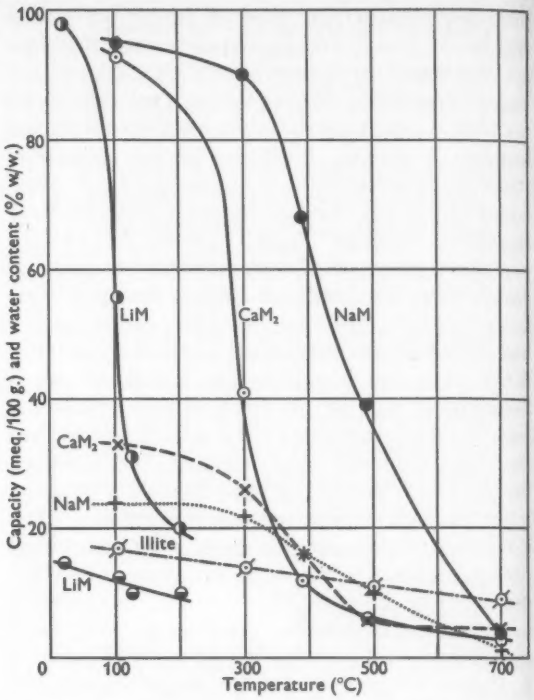

FIGURE 2 – *Water loss and decrease in capacity on heating.* ◑, ○, ●, ⌀ *Capacity;* ◒, ×, + *Water content* (M *is montmorillonite*)

THE EFFECT OF HEATING UPON CAPACITY

Water plays an important role in ion exchange in the expanding interlayer exchangers, and we would therefore expect marked changes in capacity to occur on heating. When heated to high temperatures, water is lost irreversibly from hydroxyl groups within the structure; the interlayer spacing and the cation exchange capacity decrease together. If the temperature is raised sufficiently, the cations migrate into the lattice itself, where they are firmly held against further exchange in a mullite-type structure which is no longer free to swell and allow exchanging cations to enter. The temperature at which these changes occur depends upon the cation originally present (figure 2), and there is a close correlation between water loss and decrease in capacity. The smaller cations are free to move inside the lattice at lower temperatures than are the larger ones, and consequently Li^+ ion is 'fixed' at a lower temperature than is Na^+ ion; the non-expanding mineral illite shows a much less marked change on heating.

ANION EXCHANGE IN CLAYS

Anion exchange, which is generally of less importance, may arise in two ways. Firstly, surface hydroxyl groups may be replaced by other anions; secondly, the layers may grow by incorporation of other anions of suitable geometry. Thus phosphate, arsenate, and borate ions, all of which are tetrahedral, may be added to the edges of the tetrahedral silicate layers; ions such as nitrate, chloride, or sulphate are not exchanged. Anion exchange is thus essentially an 'edge and corner' reaction; consequently we find that in kaolinite the anion and cation exchange capacities are comparable, while in montmorillonite the cation exchange capacity is greatly predominant. In the case of the phosphate ion other non-exchange mechanisms may participate, providing an increased uptake of this important plant food. Thus, in neutral and mildly acid soils it will be precipitated by the trivalent oxides (Fe_2O_3, Al_2O_3) which are always present in clays, while in alkaline soils it will be precipitated as basic phosphates of the alkaline earths (e.g. Ca^{2+} in calcareous soils).

THERMODYNAMICS OF ION EXCHANGE ON CLAYS [5].

The equilibrium between two ions and a clay exchanger may be represented quantitatively by

a typical mass-action expression. Thus in the exchange between caesium and sodium ions on montmorillonite (represented by M)

$$CsM + Na^+ \rightleftharpoons Cs^+ + NaM$$

the thermodynamic equilibrium constant K is given by the expression

$$\frac{[\overline{Na^+}][Cs^+]}{[Na^+][\overline{Cs^+}]}$$

where $[X^+]$ represents the activity of the ion X^+ in solution and $[\overline{X^+}]$ the activity in the solid phase. In dilute solutions the activities may be replaced by molar concentrations for univalent-univalent exchanges, and appropriate corrections may be made in other cases, but values of the activity coefficients in the solid phase are uncertain, and exact thermodynamic data are difficult to obtain. Until recently it has been difficult to obtain reproducible data on clay exchange equilibria, but by using standard reference clay minerals of known composition (provided by the American Petroleum Institute) H. C. Thomas has developed a sound and self-consistent treatment of the subject. Several systems have been studied on montmorillonite and on attapulgite, and Table II illustrates how the affinity series for alkali metals on attapulgite has been given quantitative meaning. The possibility of extending this treatment to the mixed clay systems found in soils is indicated by further work at Harwell [6]; an example of isotherms obtained in this work is given in figure 3.

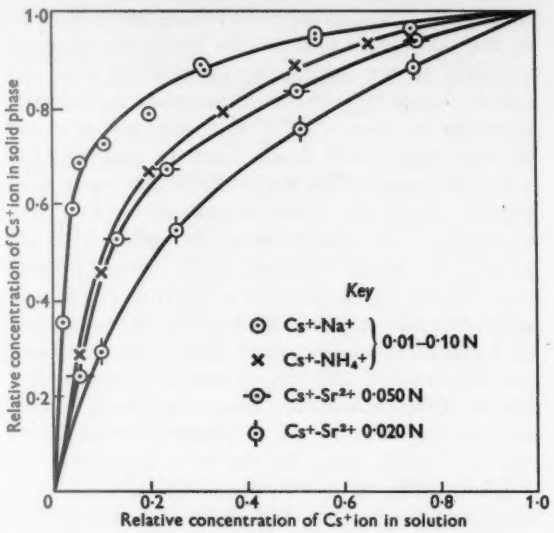

FIGURE 3 – *Representative exchange isotherms on Lower Greensand soil.*

SOME RECENT APPLICATIONS

The uptake and fixation of cations by clays and soils has acquired a new importance in connection with the treatment and disposal of radioactive wastes from the atomic energy industry. This in turn has stimulated a renewed interest in the problem of the movement of ions through the soil, and the subject has thus returned to its starting-point of more than a century ago.

Radioactivity may enter the soil either deliberately as a result of waste disposal or as fallout from a nuclear explosion, or accidentally by leakage of radioactive solutions or from a reactor accident. The activity may be either partly or wholly in soluble ionic form, depending upon its origin and upon the chemical properties of the soil. Ion exchange within the soil can play an important role in delaying the forward movement of dissolved activity and so restricting the spread of contamination. An obvious corollary to this behaviour is that the activity will remain concentrated in a limited area, within which it is readily available to plants and hence also to animals and to man unless preventive steps are taken.

Use has been made of this property of the soil for the 'ground disposal' or 'ground storage' of radioactive waste solutions at certain American atomic energy sites which are situated in relatively unpopulated areas where geological and climatic conditions are also favourable [7]. Even in the vast areas of the American continent indiscriminate disposal is not possible, and the volumes and total activity discharged must be carefully adjusted

TABLE II

Alkali metal ion exchange equilibria on attapulgite
(Merriam and Thomas, *J. Chem. Phys.*, **24**, 993, 1956)

Equilibrium (A is attapulgite)	Thermodynamic equilibrium constant, K		Free energy change $\Delta G = -RT \ln K$	Heat of exchange ΔH (30–75° C)
	30° C	75° C		
$Li^+ + CsA \rightleftharpoons LiA + Cs^+$	0·029	0·068	2·14 kcal. (30° C)	4·1 kcal.
$Na^+ + CsA \rightleftharpoons NaA + Cs^+$	0·041	0·090	1·93 (30° C)	3·8
$K^+ + CsA \rightleftharpoons KA + Cs^+$	0·131	0·243	1·22 (30° C)	2·9

as a result of searching preliminary investigations in the laboratory and in the field; the ultimate criterion is the maximum permissible level for safe discharge to public water supplies. Laboratory experiments show clearly that the cations move forward much more slowly than the movement of water as measured by the rate of anion movement (figure 4); if the delay is sufficiently long the activity of the cations may be reduced to an acceptable figure. A much more complex situation obtains in the case of a mixture of cations, where the movement of each cation depends upon its concentration, the total ionic concentration, and the relative affinities of the exchange centres for the different cations. Here the results cannot be predicted, but must be evaluated in laboratory and field trials, using the wastes in question.

The affinity series for cations on clays and soils follows approximately the Hofmeister series, that is the affinity increases with increasing cationic charge and with decreasing hydrated ion radius, although there are many exceptions to this rule. On introducing a mixed waste into the ground the rates of travel are found to increase in the following order:

$$Pu^{4+} < La^{3+}, Ce^{3+}, \text{ etc.} < Sr^{2+} < Cs^{+}$$

The more rapid movement of strontium and caesium, coupled with their long half-lives and the high biological toxicity of ^{90}Sr, implies that the quantity of these in the waste must be carefully controlled, and some form of chemical pretreatment may be necessary. Of the other species usually present in wastes, nitrate ion travels far ahead of the cations, with ruthenium intermediate between it and the cations. This is because, although Ru^{3+} ions behave similarly to the other trivalent ions, much of the ruthenium in waste solutions is present in complex uncharged or anionic species, which therefore travel much faster than the cations. Use is made of the faster movement of nitrate and of ruthenium in monitoring the spread of contamination; when these are detected in suitably located monitoring wells the discharge point is closed down, with the knowledge that the main activity front may be up to two years distant relative to the rate of movement through the soil. The failure of soils to retain nitrate ions can itself have severe consequences owing to their toxicity in high concentrations; several fatalities among the turtle population at Oak Ridge have been traced to this cause.

If the composition of the waste solution being discharged at one point is altered, this may result

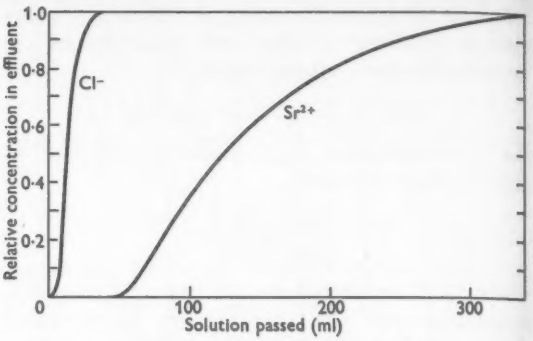

FIGURE 4 – *Breakthrough of Sr^{2+} and of Cl^{-} ions on passage through soil columns* (*Kaufman, W. J.*, et al., *Tech. Inf. Div., Oak Ridge, TID*-7517, 207, 1956).

in an accelerated movement of the ions already exchanged, particularly if the acidity is increased. In this case also the relative rates of movement of cations may be altered, since some are more easily displaced by hydrogen ions than are others (figure 5); strontium is particularly susceptible, an important fact in view of its toxicity. High concentrations of inactive salts will also reduce the uptake of activity by competing for the exchange sites. These facts emphasize the dynamic nature of this method of waste disposal and underline the need for careful evaluation of methods.

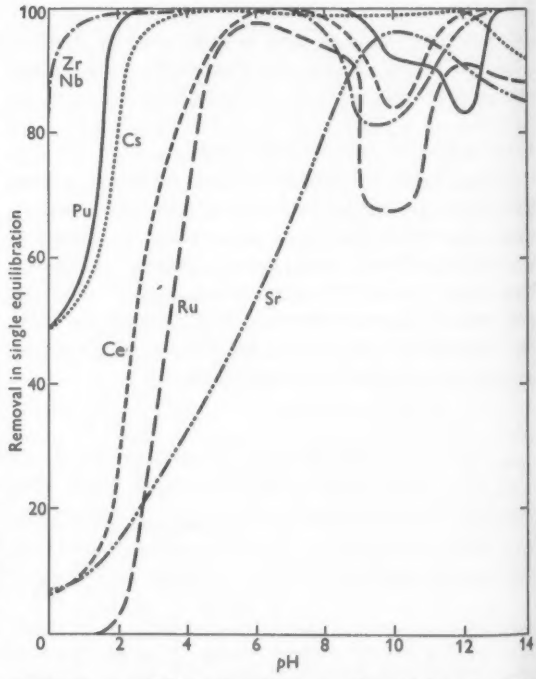

FIGURE 5 – *Effect of acidity on the uptake of certain cations by Hanford, Washington, soil* [7].

TABLE III

Removal of activity by slurrying with clay [8][1]

Activity	Ionic form	*p*H	% removal			
			p.p.m. clay 1000	2000	3000	4000
^{89}Sr–^{89}Y	Cationic	7·7	83·4	89·1	92·9	95·0
^{140}Ba–^{140}La	Cationic	7·8	88·8	92·0	94·3	97·1
$^{141,144}Ce$–^{144}Pr	Cationic	8·0	99·7	99·8	99·9	99·9
^{95}Zr–^{95}Nb	Cationic and colloidal	7·5	98·0	99·1	99·4	99·6
^{106}Ru–^{106}Rh	Mixed	5·2	50·5	59·3	61·5	—
Aged fission products	Mixed	8·8	82·0	82·9	86·3	90·3
^{131}I	Anionic	7·5	4·9	5·0	3·4	3·4

[1] Local shale from Oak Ridge; capacity 29 meq./100 g.

It has been suggested that natural ion exchangers should be used as a final stage in the treatment of low-activity effluents, in order to remove ions such as Cs^+ which are not affected by conventional precipitation and flocculation treatment. Although soils of suitable capacity are plentiful, they cannot be used in large beds with the flow rates required. The mineral vermiculite has, however, excellent porosity and packing properties combined with a useful capacity (table I); laboratory trials have demonstrated good decontamination factors, and larger-scale trials are now in progress at Harwell.

The decontamination of water by batch slurrying with clay has been proposed for the treatment of contaminated water supplies in the field [8]. This method would be unsuitable for the routine treatment of radioactive effluent, since the quantities of clay required would lead to undesirably large volumes of active sludge requiring dewatering and disposal. Removals obtained in single batches are given in Table III. In an emergency, sufficient reduction of activity could also be obtained by percolation through a column of soil.

The use of clay has also been suggested [9] for disposal of highly active waste, by absorbing the ions and then fixing them against leaching by firing to temperatures of ~ 1000° C. Advantage is taken of the ease with which ion-exchange operations can be carried out by remote control, an essential requisite for processing of highly active materials. The original liquid waste is converted into a solid form, which seems preferable for the centuries-long storage required for the decay to negligible levels of ^{137}Cs and of ^{90}Sr. This process has been successfully developed on a pilot plant scale both at Harwell and at Brookhaven, although it is doubtful whether it will be adopted on a larger scale except possibly for relatively pure active species free from inactive diluents.

In conclusion, it is obvious that the data obtained in relation to waste disposal, and the fundamental studies which are being carried out in order to provide a background for this work, cannot fail to be of interest to all whose work touches on this subject. The movement and retention of ions within the soil, the concentration of trace elements by ion exchange, and the equilibria between the soil constituents and the ions dissolved in the soil water are all of fundamental importance to workers in many fields.

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Chloroplast structure and function

J. B. THOMAS

The achieving by man of a reasonable efficiency in the conversion and chemical storage of solar energy would be an event of major importance. The study of the mechanism of photosynthesis, in which this problem is naturally solved, should give information that would help towards this achievement. By combining electron microscopy with other physical and chemical techniques considerable progress has been made in localizing the photosynthetic centres of the plant cell. These methods and some of the results are reviewed here.

Attempts to utilize solar energy with reasonable efficiency by converting and storing it chemically meet with the difficulty of the rather quick loss of the excitation energy. Therefore the main problem concerns energy storage rather than energy conversion. E. I. Rabinowitch [1] pointed out that transfer of an electron from a molecule excited by the absorption of light energy to an atomic nucleus of a different molecule offers the best chance for energy storage, provided the electron affinity of the 'acceptor' is much lower than that of the 'donor'. In this way there is sufficient time for enzymatic stabilization or spatial separation of the primary products. In other words, oxidation-reduction reactions against the electrochemical potential gradient are suitable for providing conditions required for the chemical storage of light energy.

Evidently nature has solved the problems in question quite satisfactorily. Solar energy is converted into chemical energy by the reduction of carbon dioxide to carbohydrates with the help of 'photosynthetic' pigments. Therefore a study of the natural photosynthetic apparatus may provide some insight into the reason why the process, or rather chain of processes, under consideration proceeds with a reasonable efficiency in the living cell.

The action spectrum of photosynthesis shows maxima and minima corresponding to those of the absorption spectrum of chlorophyll. It is therefore clear that this pigment absorbs photosynthetically active light. Moreover, the occurrence of additional maxima in the action spectrum demonstrates that light absorbed by carotenoids and, in red and blue-green algae also by phycobilins, is utilized in photosynthesis too. A detailed discussion of the pigments is beyond the scope of the present review, but may be found, with other more detailed information about photosynthesis, in the excellent book of Rabinowitch [2]. It will suffice to mention that in higher plants two chlorophylls occur, chlorophylls *a* and *b*, and that the ratio *a* : *b* is about 3 : 1. Photosynthetic bacteria contain related compounds: bacteriochlorophyll (purple bacteria) and bacterioviridin (green sulphur bacteria).

Excited chlorophyll molecules release their excitation energy in three ways: as heat, fluorescence, and chemical energy. Since the conversions are random processes, fluorescence can be used as a quantitative indicator of the excitation energy, and this has been done by many authors. The most detailed quantitative study on this subject is that of L. N. M. Duysens [3]. If green plants are irradiated, only chlorophyll *a* fluorescence is observed. Calculation showed that excitation energy of light absorbed mainly by the carotenoids is transferred to chlorophyll *a*. The efficiency of this transfer, however, is relatively low (20–40 per cent). Previously H. J. Dutton, W. M. Manning, and B. M. Duggar [4] and E. C. Wassink and J. A. H. Kersten [5], independently of each other, had found with diatoms that light energy absorbed by the carotenoid fucoxanthin is as active in photosynthesis as light absorbed by chlorophyll. Fluorescence experiments showed further that energy absorbed by fucoxanthin enters the chain of chemical 'dark' processes in photosynthesis through chlorophyll. In this case a transfer of excitation energy occurs with an efficiency approaching 100 per cent. Others found that the same is true of such transfers from phycobilins to chlorophyll as well as from chlorophyll *b* to chlorophyll *a*, or, in purple bacteria, from bacteriochlorophyll types 'B800' and 'B850' to type 'B890'. The occurrence of such high efficiencies suggests that the transfer mechanism involves inductive resonance [6]. It may be added that the low-efficiency transfers first mentioned can be readily explained by the fact that the 'overlap' of the spectra of the carotenoids in question with that

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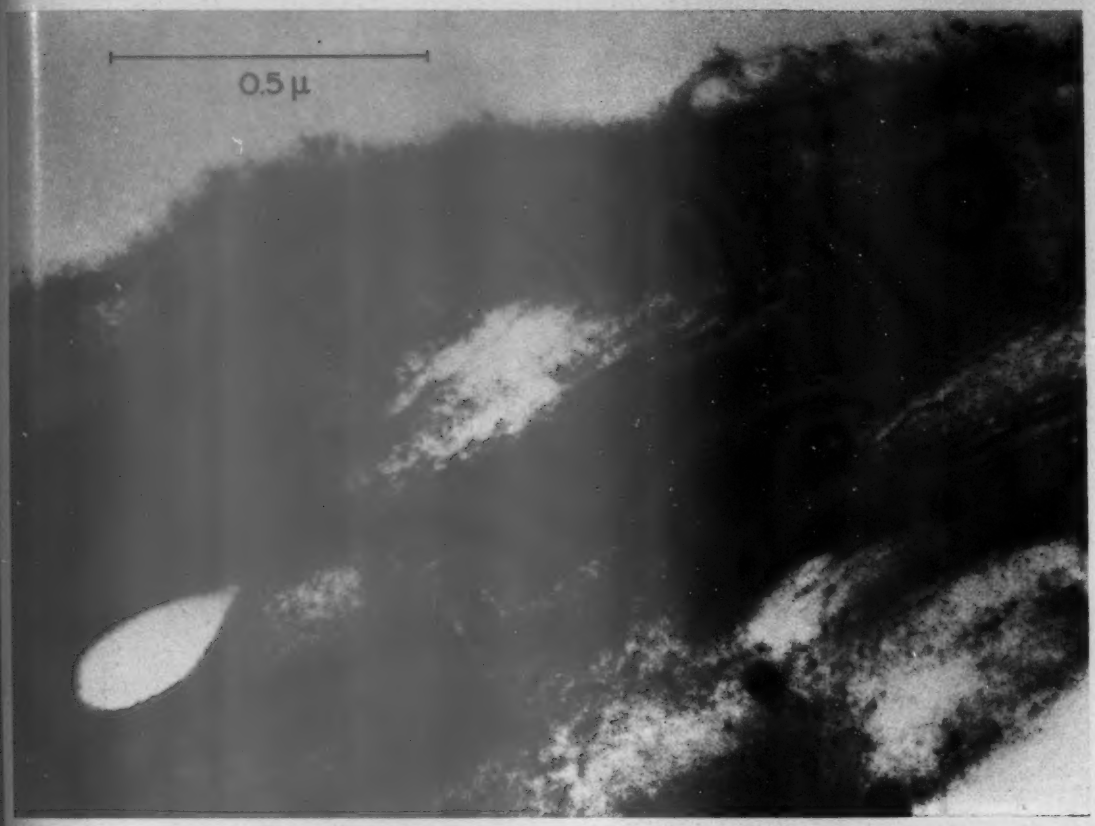

FIGURE 1 – *Detail of chloroplast of* Elodea densa. (*Electron micrograph by courtesy of P. F. Elbers.*)

of chlorophyll is poor, whereas in the other cases mentioned a good overlap occurs.

Apart from the spectral requirements, pigment concentration must be sufficiently high for efficient energy transfer to occur, and the transfer will be facilitated by suitable orientation of the pigment molecules. Hence the state in which the pigments occur in the living cell is of primary importance for their functioning. The data concerning this state will be briefly discussed first.

If pigments are extracted by grinding the photosynthetic cells in water, the absorption spectrum of the colloidal extract is the same as that of the intact cells. However, extraction with organic solvents, including alcohols, ether, acetone and others, yields a clear solution and the absorption maxima are shifted as compared with those of the spectrum *in vivo*. This proves that the state of these pigments in the living cell differs from that in solution.

This difference of state is also demonstrated by the fact that chlorophyll fluorescence is much weaker *in vivo* than in solution. It is indeed most curious that chlorophyll fluoresces at all in the natural state. If a solution of this pigment in an organic solvent is prepared of the same concentration as occurs in the chloroplast (about 0·2 M), concentration quenching will extinguish any observable fluorescence. It is also observed that a colloidal dispersion of chlorophyll in water is non-fluorescent. Why chlorophyll in the aqueous medium of the cell is capable of fluorescence remains to be explained.

For various reasons photosynthetic pigments in their natural state seem to be bound to proteins, or rather lipoproteins. One of the functions of these protein-pigment complexes is evidently the establishment of conditions required for the functioning of the pigments, e.g. by establishing some definite spatial arrangement. As a rule, the protein-pigment complexes are incorporated in distinct cell structures, the chloroplasts. The

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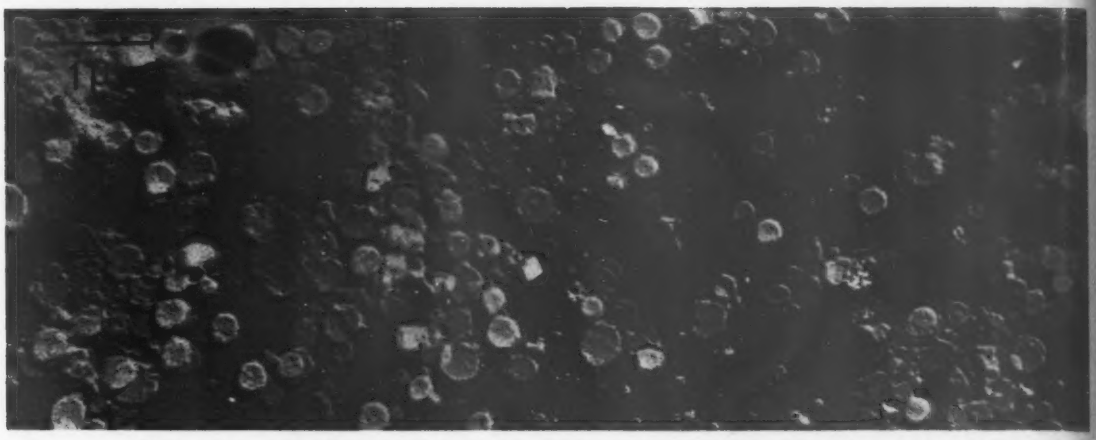

FIGURE 2 – *Electron micrograph showing isolated whole and fragmented grana from chloroplasts of* Spinacia oleracea.

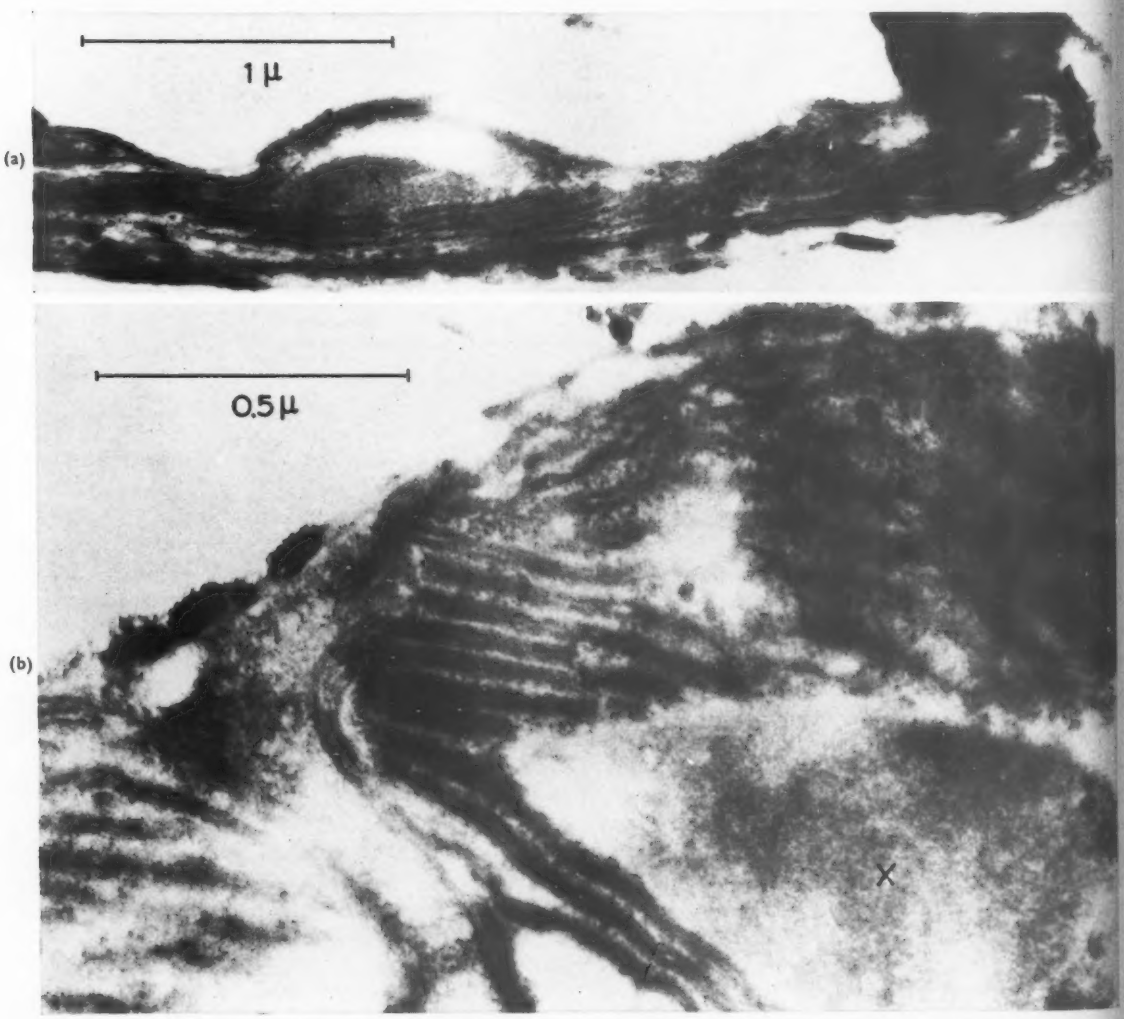

FIGURE 3 – *Chloroplast of the diatom* Nitzschia dissipata. (a) *General view.* (b) *Detail. Product of photosynthesis denoted* x *in both. (Electron micrograph by courtesy of P. F. Elbers.)*

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structure of these organelles may be discussed first.

Chloroplasts contain lamellae, of a thickness of roughly 50 Å, embedded in a globular material, the stroma, and are most probably surrounded by a double membrane with semi-permeable properties. Two main types can be distinguished: 'granular' chloroplasts, which seem to be typical of higher plants, and 'lamellate' ones. Figure 1 shows an electron micrograph of a section through the granular chloroplast of *Elodea densa* in which the components mentioned can be seen. In various regions the contrast as well as the compactness of the lamellae is increased. This increase in contrast is due to the fact that these regions are more heavily 'stained' by the strongly electron-scattering osmic acid during preparation than the remaining parts of the lamellae. It indicates that the dark regions are rich in lipids. If such chloroplasts are damaged after isolation from the cell, e.g. by osmotic effects, they become readily disintegrated except for these dark regions. The latter become freed from the chloroplasts and, as shown in figure 2, show up as disks. Each disk represents a stack of circular lamellae. As can be seen, some stacks are disintegrated into isolated lamellae. The lamellar stacks which, more or less, represent structural units are called 'grana'.

In the chloroplast, the grana are embedded in the 'stroma substance'. It consists of a globular proteinaceous substance through which lamellae connecting the grana are extended. These lamellae, or rather parts of the lamellae, are called 'stroma lamellae' as opposed to the 'grana lamellae'.

The lamellate chloroplasts do not contain grana. As a rule, the lamellae are evenly 'stained' with osmic acid throughout these chloroplasts, while no regions at which the lamellae seem to be relatively closely packed are observed. These chloroplasts are often found in algae. Figure 3 shows such a chloroplast of the diatom *Nitzschia dissipata*. As often found in lamellate chloroplasts, the lamellae occur in bundles of from 4 to 8. The globular stroma occurs in relatively thin layers between these bundles. Stroma lamellae are absent.

In a few cases forms intermediate between both types of chloroplasts were observed, namely in *Spirogyra* by T. Butterfasz [7] and in some Desmidiaceae by R. Chardard and C. Rouillier [8]. For reviews of studies on chloroplasts the reader may be referred to H. Leyon [9], D. von Wettstein [10], and J. B. Thomas [11].

In blue-green algae and in photosynthetic bacteria no true chloroplasts occur. In the former organisms W. Niklowitz and G. Drews [12] observed lamellae situated in the cytoplasm of the cells. Neither chloroplast membrane nor special stroma was observed. The authors call a structure such as this, which is probably photosynthetic, 'chromatoplasm'. In photosynthetic bacteria 'free' grana occur. The same authors [13] were the first to present a beautiful electron micrograph of organelles—from the purple bacterium *Rhodospirillum rubrum*—that most likely are free grana. The lamellate structure was evident. No surrounding membrane was observed.

In conclusion, it can be said that the structure of the photosynthetic apparatus may be different in different species but that lamellae occur in all. This statement strongly suggests that the presence of lamellae is a prerequisite for photosynthesis. Some data concerning these structures are therefore discussed below.

The resolving power of the light microscope is such that grana can be observed only if their diameter exceeds 0·3 μ. Examination of chloroplasts containing grana of these or larger dimensions shows that these grana are green. The surrounding stroma, however, is colourless. Moreover, under the fluorescence microscope the grana exhibit a red fluorescence, while the stroma remains dark. These observations may demonstrate that the occurrence of chlorophyll is restricted to the grana. Since in aqueous extracts the carotenoids are found always to accompany the chlorophyll-protein complexes, most probably the same can be said of these accessory pigments.

An attempt [14] to localize the chlorophyll more precisely was made as follows. The presence of chlorophyll can be demonstrated by the Molisch reaction, in which silver nitrate is reduced to metallic silver by illuminated chlorophyll. This reaction offers a possibility of locating the chlorophyll more exactly. A suspension of grana is vibrated sonically at 7000 c/s until the grana are disintegrated into lamellae. To the suspension silver nitrate is added, and the mixture is illuminated. Electron-microscopical examination shows that silver deposits are formed at the surface of the lamellae. It may therefore be assumed that the lamellae function as carriers of chlorophyll and probably also of the accessory pigments.

The same conclusion can be drawn in a less direct way from the fact that a suspension of lamellae is capable of photolysing water. As soon as granular chloroplasts are fragmented, their ability to photosynthesize is lost, but in the presence of

a suitable hydrogen acceptor they are capable of photochemical liberation of oxygen from water. The action spectrum proves that this 'Hill reaction' is mediated by excited chlorophyll.

If such a suspension is further vibrated, the lamellae become fragmented. Even these fragments are capable of the Hill reaction. During the disintegration the reaction rate at first remains fairly constant but then drops rather quickly. This occurs at a particle volume of about 10^6 Å^3 [15], and it has been calculated that these particles contain roughly 100 chlorophyll molecules. At a volume of 2×10^5 Å^3 the ability to perform the Hill reaction is totally lost. This means that at these dimensions all chlorophyll molecules are removed from the enzymic centres at which the products of the Hill reaction are formed with the aid of the excitation energy from the chlorophyll. It also means that at a volume of about 10^6 Å^3 the enzymic centres are provided with their complete number of active pigment molecules.

A. Frey-Wyssling and E. Steinmann [16] pointed out that, in good electron micrographs, the lamellae can be seen to consist of spherical particles. The authors suggested that these particles represent protein macromolecules. It remains to be seen whether the globular shape is natural or not. During fixation of electron microscopical preparations, spirally shaped macromolecules may well curl up and attain a spherical form. Anyway, it seems very likely that the lamellae are built up of macromolecules. The dimensions of these macromolecules proved to be of the same order of magnitude as those of the smallest particles still fully active in the Hill reaction.

The above data demonstrate another function of the grana lamellae, namely the carrying of enzymic centres. However, there are strong indications that not all the photosynthetic enzymes are associated with the lamellae. This may be suggested by the fact that it was found to be possible to restore photosynthetic activity of isolated spinach chloroplasts, damaged by treatment with water to such an extent that the CO_2-fixing capacity was lost, by addition of an enzyme extract from whole chloroplasts [17]. A comparison of fluorescence induction phenomena in granular and lamellate chloroplasts [18] shows that part of the photosynthetic enzymes are probably located in the stroma.

Estimations of both total surface of grana lamellae and chlorophyll content of chloroplasts from various plant species showed that in all cases the lamellar area available per chlorophyll molecule was about the same, namely a few hundred square ångströms. Since transfer of excitation energy with an efficiency of 96 per cent between chlorophyll *a* and *b*, or between the bacteriochlorophyll types mentioned earlier, is possible if each molecule of these pigments occurs within an area of roughly 10^3 Å^2, it is evident that the area available per chlorophyll molecule is such that a highly efficient energy transfer is possible. This is also true in lamellate chloroplasts if the chlorophyll is assumed to be evenly distributed over the lamellar surface throughout the whole chloroplast. Because of the fact that the total lamellate chloroplast shows a red fluorescence under the ultra-violet microscope, it seems likely that such a homogeneous distribution of chlorophyll occurs. This conclusion is supported by the electron-microscopical observation that the total area of lamellae from this type of chloroplast is evenly stained with osmium tetroxide. With 'intermediate' chloroplasts [7] the above may be only partly true.

As the pigment-carrying lamellae are thin layers, their thickness being about 50 Å, the question arises whether the pigments occur in an orientated state. It was mentioned before that orientation may facilitate energy transfer. Earlier investigations, in which dichroism was measured, yielded contradictory results. J. C. Goedheer [19], however, pointed out why measuring of dichroism is not suitable for studying pigment orientation in these particular objects. He studied dichroism (anisotropy of absorption) in combination with birefringence (anisotropy of refractive index) and polarization of fluorescence by chlorophylls both *in vitro* and *in vivo*. These experiments led to the following conclusions. First, in the chloroplast the chlorophyll molecules are probably concentrated in thin films, occupying less than 7 per cent of the cross-section of a lamellate chloroplast or granum. Second, combination of experiments both *in vitro* and *in vivo* showed that, most probably, the oscillator responsible for the red absorption band of chlorophyll has a preferred orientation in the direction of the lamellae. The same is true of the 'infra-red oscillator' of bacteriochlorophyll. As, moreover, experimental evidence was obtained that the oscillators in question are orientated in the direction of the shortest dimension of the conjugated ring system which constitutes the porphyrin 'head' of the chlorophyll and bacteriochlorophyll molecules, it could be concluded that, most probably, the porphyrin rings of these

pigments are preferentially orientated in the plane of the lamellae.

The orientation, however, was found to be relatively poor. As an explanation for this phenomenon Goedheer suggested that the pigment molecules may be arranged at the 'curved' surface of the earlier-mentioned spherical or spirally shaped lipoprotein macromolecules.

In grana or in lamellate chloroplasts the lipoprotein layers, the lamellae, alternate with layers of 'free' oriented lipids. Experiments on staining with rhodamine B suggested that the carotenoids are not arranged among these free lipids. Thus they probably are located, together with the chlorophyll molecules, at the surface of the lipoprotein layers.

Summarizing the available experimental evidence, it can be said that the lamellae of grana and lamellate chloroplasts are carriers of the photosynthetic pigments and, as such:

(1) make possible the functioning of the photosynthetic pigments in the aqueous medium of the cell by providing the required complex of photosynthetic pigments with their associated lipoproteins;

(2) ensure the maintenance of intermolecular distances at which an efficient energy transfer is possible. This may be important, because changes in the water content of the chloroplasts may occur during photosynthetic formation of osmotic substances. Thus the intermolecular distances of the pigments might considerably increase if these substances occurred in some more or less 'dissolved' state. Such a situation might be imagined to occur if the pigments were rendered soluble, e.g. by combination with some other substance;

(3) may facilitate energy transfer by orientation of the photosynthetic pigments; and

(4) are carriers of enzymic centres involved in the first steps of the photosynthetic reaction chain and thus guarantee a close connection between these enzymic centres and chlorophyll molecules under possibly varying conditions (*cf.* under 2).

Another function of the lamellae was suggested by Leyon [9] and von Wettstein [10]. The occurrence of more or less closely packed lamellae may give rise to the occurrence of flattened capillary channels extending throughout the chloroplast. Along the walls of these channels the photosynthetic products may be transported quickly. It certainly would be worth while to try to get experimental confirmation of this hypothesis.

With regard to the problem initially mentioned, concerning the way in which excitation energy is efficiently stored in the photosynthetic apparatus, the above considerations suggest that the lamellar structures discussed may provide at least two essential requirements for this process: firstly, adequate intermolecular distances for an efficient energy transfer down to some enzymic centre are maintained, and, secondly, the first photoproducts may be quickly transported and thus may be prevented from recombining. However, much more research is needed to settle this question definitely.

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Book reviews

EUCLIDEAN GEOMETRY

Problems of Euclidean Space, *by H. G. Eggleston. Pp. viii + 165. Pergamon Press Ltd, London.* 1957. 40*s. net.*

This short, expensive, and extremely interesting book was the Adams Prize Essay of the University of Cambridge for the year 1955–56. It gives an admirable account of the concept of convexity in contemporary mathematical research. Typical problems which are studied in detail are geometrical properties for which triangles are the extremal convex curves, and the asymmetry of curves of constant width. In the early chapters the problems are of a general nature, and the major difficulty is that of conjecturing the correct result. In the last chapter the correct result can often be guessed without difficulty by those who are expert at the subject, but the real difficulty is then to establish the truth of the conjecture.

The problems of convexity are developed from several distinct points of view: (1) the consequences of the fact that a closed convex set is an intersection of closed half-spaces; (2) the behaviour of convex sets as particular types of connected sets; (3) the discovery of extremal properties for particular convex sets; (4) the relation between simple geometrical figures and general convex sets as a direct approximation; and (5) the discovery of problems in which convex sets in two or three dimensions are the appropriate generalizations of intervals in one dimension.

The treatment throughout is very sophisticated but very rewarding, and this little book will be of the utmost value to those who wish to learn the results of the most recent research in this fascinating branch of modern pure mathematics.

G. TEMPLE

STATISTICAL MECHANICS OF FLUIDS

Mécanique statistique des fluides, *by D. Massignon. Pp. xiv + 264. Dunod, Paris.* 1958. *Fcs* 3900 *net.*

This book is sponsored by the French National Council of Scientific Research and is part of the mathematical series directed by Professor A. Lichnerowicz of the Collège de France. It uses elaborate vectorial writing and is a systematic account of general formulations common to all statistical theories of more or less condensed fluids; perfect gases or critical states are studied only as special cases.

Local measurements, even so-called instantaneous ones, give only mean values for systems of particles subject to thermal agitation, values that are more or less refined or loaded in favour of the point and moment aimed at. Using the essential notions of dispersion and correlation, hydrodynamic values are correctly defined in Euler variables, and lead to the density, pressure, and mass stream fluctuations corresponding to fluid temperature and thermodynamic characteristics. Such fluctuations, while generally not directly accessible, increase near the critical point.

Quantic hydrodynamics and statistics are developed in the last chapter for boson and fermion systems so as to explain the large critical volume and low critical temperature of hydrogen and helium.

Appended definitions, formulae, and references make things easier even for readers unaccustomed to statistical mechanics.

E. S. TRAVERS

ELECTRON IMPACT PHENOMENA

Electron Impact Phenomena and the Properties of Gaseous Ions, *by F. H. Field and J. H. Franklin. Pp. ix + 349. Academic Press Inc., New York; Academic Books Ltd, London.* 1957. $8.50 *net.*

This book contains an extensive and critical compilation of data on electron impact phenomena. It reviews the properties of gaseous ions with particular reference to the chemical physics of the systems studied. The recent increase in the amount and variety of information in this field has been largely due to the availability of commercial mass spectrometers, and a major part of the book is concerned with the results of such studies. However, the treatment is general and is concerned with the tabulation and interpretation of all available energetic data pertaining to electron impact phenomena. The importance of such data in connection with molecular structure and chemical behaviour is stressed and recent theoretical treatments of ionization potentials and fragmentation processes are given. A chapter is devoted to the relevance of the ionization potentials of gaseous organic molecules to the mechanisms of reactions in solution which proceed through ions or ion pairs. Attention is also called to the possible significance of this type of data in radiation chemistry and catalysis. The book serves to summarize and collate the information in a field of growing importance.

W. G. PRICE

CHEMICAL THERMODYNAMICS

An Introduction to Chemical Thermodynamics, *by E. F. Caldin. Pp. xv + 424. Clarendon Press, Oxford.* 1958. 50*s. net.*

This is a very straightforward introduction to chemical thermodynamics, based on the Gibbs method. This involves introducing the basic equation of Gibbs, viz. $dE = dq + dw + \mu_1 dn_1$, on page 29. This leads at once to the definition of chemical potentials, but the condition $dE = \mu_1 dn_1$, in which a substance is added without gain of heat and with no work done, is somewhat difficult to realize. I think that students who are not very analytically minded may have difficulty here if they have no previous knowledge of the subject. However, granted this approach, the subject is developed very clearly and logically, and a reasonable amount of illustrative data is given. Every author has difficulties with arrangement, and often what is gained in one direction is lost in another. At the same time it seems a little difficult to deal with galvanic cells as a means of determining standard potentials of ions on pages 252–68, when the activity coefficients of electrolytes in solution are not reached until the final very short chapter of only twenty pages, at page 386. The price fixed by the publishers is disturbingly high for what is obviously intended as a students' textbook.

J. A. V. BUTLER

RADIOISOTOPE LABORATORY TECHNIQUES

Radioisotope Laboratory Techniques, *by R. A. Faires and B. H. Parks. Pp. xii + 244. George Newnes Ltd, London.* 1957. 25*s. net.*

This book briefly describes the kinds of laboratory, apparatus, and technique required by the use of radioactive isotopes. The first chapters deal in an elementary way with the production and properties of radioactive isotopes, accommodation and equipment needed for their safe manipulation, and the instrumentation required for their detection and assay. The final chapters give an idea of how radioactive isotopes

may be used as tracers and as sources of ionizing and penetrating radiation. Appendices provide data such as count-rate losses for the single paralysis time of 400 μsec, an energy-range curve for mono-energetic electrons, and a selected list of pile-produced radioactive isotopes and their properties.

The book is varied in quality. On one hand it contains some useful and up to date practical information on health hazards, laboratory accommodation, and manipulation techniques; thus a novel and useful demountable filter is described for the collection of geometrically uniform precipitates. On the other hand, the treatment of principles is inadequate. For example, the description of radioactivation analysis (p. 198) is very misleading on account of the unfortunate use of the term 'constituent' where 'element' would have been better. The book would have been greatly improved by a little more care in editing. It is recommended not as a general introduction to radioisotope techniques but as a useful book for the intelligent worker who has a clear idea of how radioisotopes may be helpful but who is not clear about the purely practical applications in the laboratory. F. P. W. WINTERINGHAM

NATURAL QUINONES

Naturally Occurring Quinones, *by R. H. Thomson. Pp. vii + 302. Academic Press Inc., New York; Butterworths Scientific Publications, London.* 1957. 50*s. net.*

The last comprehensive account of the quinones occupies some 59 pages in Mayer and Cook's 'The Chemistry of Natural Colouring Matters' published in 1943. The present volume is an indication of the rapid extension of our knowledge: it describes over 150 natural quinones isolated from plants, fungi, lichens, insects, and sea urchins, ranging in complexity from *p*-benzoquinone to the nonacyclic erythroaphins. Although the majority of these quinones contribute little to natural colouring, this is not always the case; e.g. the anthraquinone parietin (physcion) of the common yellow-orange lichen, *Xanthoria parietina*, picturesquely decorates rocks, walls and roofs in country and seaside districts in many countries.

The chapters deal with functions and biosynthesis; benzoquinones, including terphenyl- and dibenzoquinones; the large group of naphthaquinones; the anthraquinones, which include many long-known compounds used as dyes, and certain insect pigments; the few phenanthraquinones; and a final chapter on the aphins and on the photodynamic pigment hypericin. There are some 1000 references to the original literature, a good general index, and also zoological and botanical indexes.

This is a stimulating, well-written and well-printed book which is much more than a catalogue of structures. The bright orange of the dust cover seems particularly appropriately chosen, and one is tempted to see if it can be reduced to a colourless dihydro form.

W. BAKER

PHENAZINES

Phenazines, *by G. A. Swan and D. G. I. Felton. Pp. xix + 693. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. $22.50 *net.*

This is the eleventh volume in the series of monographs 'The Chemistry of Heterocyclic Compounds' edited by A. Weissberger, and it maintains the high standard set by the previous numbers. Part I, by G. A. Swan, deals with phenazines (9:10-diaza-anthracenes) which are not fused to other ring systems, and reviews their general methods of synthesis, their homologues, hydrogenated derivatives, and substitution products, with special sections devoted to hydroxy, keto, and amino derivatives. Separate chapters then follow on the safranines, the indulines and nigrosines, aniline black, the bacterial pigments derived from the phenazine nucleus, and biological properties.

Part II, by D. G. I. Felton, summarizes existing knowledge of monobenzophenazines and the related phenazinium salts and their hydroxy and amino derivatives, penta- and polycyclic phenazines, indanthrone, and phenazines which are condensed with other heterocyclic systems.

This is essentially a reference book, and each chapter has a very full bibliography; there is a comprehensive subject index. Nevertheless it is a book which can be read with pleasure, not only because of the excellence of the writing but also because of the attractive graphic formulae which are used with generosity. The authors have performed a very valuable service in condensing the extensive and diffuse literature of the phenazines into an orderly and logical review. W. BAKER

CRYSTALLOGRAPHY

Structure Reports for 1951, *Vol. XV, edited by A. J. C. Wilson. Pp. viii + 588. International Union of Crystallography. Oosthoek's Uitgevers Mij., Utrecht.* 1957. £10 10*s. net.*

This volume, which is profusely and excellently illustrated, gives details of the crystal structures of some 2000 metals, inorganic and organic compounds, analysed by means of X-ray or neutron-diffraction methods and published during 1951, together with any ancillary information of structural interest. These reports are well systematized, and indeed frequently amount to a short review in a particular field. For example, five pages are devoted to the ferro-electric $BaTiO_3$, on which thirteen papers appeared in eight different journals in 1951. Six pages are devoted to uranium compounds, ten to polysaccharides, and ten to various polymers. There is an excellent coverage of papers from Russian and other journals not easily accessible in Britain, and although, in general, investigations of texture, powder data, and electron diffraction data are not included unless of special structural interest, an exception is made in the case of information from such journals. In general the aim is to present the reader with all the structural details available (sometimes recalculated) without the need to refer to the original paper unless interest lies in the method of analysis rather than in the results of the investigation. These volumes form a series indispensable to scientists in many disciplines.

KATHLEEN LONSDALE

INTERNATIONAL GEOPHYSICAL YEAR

Annals of the International Geophysical Year. *Vol. III, pt. 1, pp. xix + 167; pts. 2–4, pp. 173–381; Vol. IV, pts. 1–3, pp. 201; pts. 4–7, pp. 207–393; Vol. V, pts. 1–3, pp. 244. Pergamon Press Ltd, London.* 1957. *Subscription per volume* £6 *net.*

The primary object of the I.G.Y. is to obtain simultaneous and comparable observations of geophysical quantities, particularly of those connected with the atmosphere. A world-wide co-operative enterprise of this kind presents a very serious problem of communication and exposition. As the participating countries are at very different levels of scientific development and speak many different languages, it is clearly a matter of some difficulty to ensure that the measurements are made and reduced correctly and that the results are presented in an intelligible, useful, and comparable form. The 'Annals of the International Geophysical Year' attempts

to meet some aspects of this problem. In his foreword to the first volume to be published, Professor Chapman, the President of C.S.A.G.I., the organizing committee of the I.G.Y., says that the Annals will 'indicate the inception, the planning, the preparations and the programmes, and in due course the progress and some of the main results of the I.G.Y.'

Volume III is an instruction manual for ionospheric observations. Part 1 is a 167-page treatise on ionospheric vertical soundings. It is particularly notable for the large number of reproductions of records which will clearly be of great value to those operating stations in remote places. Part 2 deals with the measurement of ionospheric absorption, part 3 with ionospheric drifts, and part 4 with radio noise, whistlers, meteors, and ionospheric scatter.

Volume IV part 1 gives very brief instructions for the measurement of natural and artificial radioactivity in the atmosphere and sea. Part 2 describes the methods of observing the aurora. Part 3 gives instructions for the determination of longitude and latitude. Parts 4 and 5 describe methods of making and reducing measurements of the earth's magnetic field. Part 6 describes the form in which seismological results should be presented, and Part 7 describes instruments for measuring neutron and meson flux.

Volume V part 1 is a substantial handbook on the measurement of atmospheric ozone. Part 2 describes auroral measurements with a camera covering the whole sky. Part 3 is a list of Antarctic radio stations.

Inevitably such a publication as the Annals contains very diverse material, and it is likely to be difficult for the reader to find his way to what he wants. This difficulty is greatly accentuated by the absence of either subject or author indexes. Each part contains a list of contents, but there seems no way, other than turning over the pages and finding these lists, to discover what subjects are treated in a given volume or how many parts constitute a volume. The usefulness of this very elaborate and expensive publication will be seriously restricted if a long series of volumes is produced in which it is next to impossible to find what one wants. E. C. BULLARD

EVOLUTION

A Hundred Years of Evolution, *by G. S. Carter. Pp. x + 206. Sidgwick and Jackson, London.* 1957. *21s. net.*

Dr Carter has taken the opportunity presented by the centenary of the publication of the 'Origin of Species' to provide in a small compass an attractively written semi-popular history of Darwinism. To say that it is semi-popular is to indicate that there is little in it that is unfamiliar to the professional biologist, though there are few publications in which the material is so compactly and intelligently presented. At the same time, its greatest appeal should be to the scientist in other fields of knowledge, to the intelligent layman, and to schools and schoolmasters.

Dr Carter wisely does not limit himself to the century implied in his title; the early chapters provide the framework of biological knowledge within which the 'Origin of Species' was conceived, and there is a good account of the philosophical, social, and religious climate into which this startling conception erupted. The actual controversies of the late nineteenth century are lightly sketched in, so that in the final chapters the genuine scientific difficulties encountered by the theory in the twentieth century can be explored, and a clear picture of the neo-Darwinian position emerges. A useful glossary and a brief but effective index enhance the value of the book.

J. E. HARRIS

PROTEIN CHEMISTRY

Introduction to Protein Chemistry, *by S. W. Fox and J. F. Foster. Pp. viii + 459. John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1957. *76s. net.*

A good deal of interesting material is presented here in such a form that it should be easy for students to assimilate it. Since a substantial list of selected references, both to reviews and to original articles, is included at the end of each chapter, the book is also suitable as a basis for further reading. In logical sequence it deals first with amino acids, which take up nearly a quarter of the total number of pages, and then with peptides; there follows a treatment of the properties of proteins in general, and a considerable number of individual proteins is also described. The authors have evidently tried to make the book readable and avoid complexity. The sections which, with a minimum of mathematics, describe physical methods for investigating proteins will be particularly useful to many whose scientific backgrounds are mainly biological or medical. The book contains a generous number of figures, some of which admirably illustrate special techniques. Others, however, are superfluous; how much of significance will the student learn from figure 18-4, which consists of photographs of bottles labelled with the names of human blood plasma fractions? Figure 8-1, which depicts a paper electrophoresis apparatus, is referred to as illustrating paper chromatography.

A personal element must always enter into the selection and weighting of subject matter for a work of this kind. To the reviewer it seems a pity that the conversion of pepsinogen to pepsin was chosen for detailed treatment as an example of the activation of a zymogen rather than the better understood and in some ways simpler case of trypsinogen or chymotrypsinogen. While five pages are devoted to haemoglobin, mention of other haem proteins is confined to a listing of some physical constants of cytochrome C. Phosphoproteins and glycoproteins are also mentioned only in passing—though it must be said in fairness that other more voluminous and comprehensive treatises do little more.

In chapter 9, dealing with the determination of the structure of peptides, no clear distinction is drawn between methods commonly used in recent work and those which are now of largely historical interest. Although a number of the latter type are included, the hydrazinolysis method for C-terminal residues is not. The surprising suggestion is made on page 148 that melting points and mixed melting points are of general applicability for the identification of peptides. The statement, on page 309, that denatured proteins give an α-keratin type of X-ray diffraction pattern is incorrect. On page 372 it is stated that the A and B chains of insulin were separated on columns; no well-known method for effecting this separation in fact employs columns. Perhaps it is unreasonable to expect a biochemical textbook today to be free of inaccuracies. There is a good deal to be said for the view that a major educational function of a young and rapidly expanding subject such as biochemistry is to dispel once and for all from the mind of the elementary student the notion of the infallibility of the printed word. F. R. JEVONS

BOTANY

Il Nuovo Pokorny, *by A. Ceruti. Pp. viii + 680. Loescher, Turin.* 1957. *Lire 4800 net.*

Pokorny's 'Illustrated Botany', first published in 1836, ran, with revisions, to some 30 editions and was a standard work for many years. The present volume is more than a simple revision: an almost completely new text has been prepared, although the original didactic method of presentation and a number of illustrations have been retained.

The aim of the book is to give a systematic account of the plant kingdom, using as far as possible common 'types' or those most typical of each group. The groups from *Thallophyta* to *Spermatophyta* are arranged according to Engler's system, and the nomenclature is brought up to date in accordance with continental usage. Each group is preceded by a short analytical key using characters intended to draw attention to its fundamental morphology. These keys are simple, practical, and with the exception of the lower groups do not require apparatus more elaborate than a hand lens. For each group a short account is given, under the heading biology, of the life history, some physiology, but little or no ecology. These accounts, although brief and oversimplified, are generally in agreement with modern knowledge. The illustrations are numerous and mostly of a high standard, particularly those specially prepared for this work by U. Tosco, though some reproductions from other authors are not of the same quality.

The author claims that this work should enable students and the intelligent amateur to obtain a wide knowledge of botany by working on their own. Although particularly suited to the teaching methods of the continental gymnasium it is probably too superficial on some parts of the subject to be of the same usefulness to British schools.

F. H. WHITEHEAD

BIRD DISEASES

Tratado de Doenças das Aves (*second edition*), *by J. Reis and P. Nobrega (with the collaboration of A. S. Reis, R. C. Bueno, and M. Giovannoni). Vol. I, pp.* 391; *vol. II, pp.* 416; *vol. III, pp.* 318; *vol. IV, pp.* 428. *Instituto Biológico de São Paulo, São Paulo.* 1956. *Cruzeiros* 950 *net.*

When this book first appeared in 1936 it was apparent that it would be the standard work for years to come. The present, long-delayed, but none the less welcome edition amply confirms this. The literature covering all aspects of avian disease has been comprehensively reviewed up to 1954, with the inevitable expansion of the book to almost four times its original size. It is now divided into four volumes, which have been paired to form two books. Most sections have been largely rewritten, and a considerable number of new illustrations have been added. The half-tones among these have suffered somewhat by being printed on a cheaper paper than that used in the first edition, but on the whole they are still good. There are bibliographies at the end of each chapter, the value of which alone make the book a necessity for the avian or comparative pathologist. For example, the chapter on Newcastle disease has 434 references and that on Pullorum disease 548, and this extensive review of the literature is maintained throughout.

Volume I deals with virus diseases and covers the field very thoroughly, even down to a virus infection of owls. The section on leucosis, which is inexplicably separated from the rest of neoplastic disease, has a good introductory section on avian haematology. There is a well-balanced review of the controversial issue regarding the relationship of the leucoses with fowl paralysis, and with the filterable tumours. The photograph of the ovarian endothelioma (figure 136) is not convincing, and there is no histology to support the diagnosis of these very debatable tumours.

An omission of some importance occurs in the section (17) which deals with diseases of uncertain aetiology. Round heart disease, although admittedly of limited geographical distribution, has a relatively abundant literature, and should surely have been included.

Volume II describes diseases due to bacteria and moulds and is excellent throughout, although the usage of *Actinomyces necrophorus* for *Fusiformis necrophorus* is a little confusing at first sight for some pathologists.

Volume III, diseases due to protozoa and arthropod parasites, contains a very welcome review of toxoplasmosis in birds, which is of considerable interest, and relevant to the increasing recognition of the widespread pathogenicity of this rather mysterious organism. The section on arthropod parasites is illustrated by excellent line drawings.

Volume IV covers the remainder of the field, i.e. helminth diseases and nutritional diseases, which are dealt with in detail, followed by briefer accounts of systemic and special organ diseases, vicious habits, poisons, developmental abnormalities, hygiene, general therapeutics, and surgery. There are eight colour plates at the end of this volume. Only a few misprints not included in the brief errata were detected, and these were of minor importance. All told, this is an authoritative and monumental work which must some day be translated for the wider benefit of the English-speaking world. It is to be hoped that there will not be such a lengthy interval before the next edition.

J. G. CAMPBELL

BLOOD

Le sang et la transfusion sanguine, *by M. Bessis. Pp. viii* + 139. *Dunod, Paris.* 1957. *Fcs* 740 *net.*

This is the first book of a series *La nature et l'homme* directed by the editor in chief of *La Nature*. *Le sang et la transfusion sanguine* is full of wonders, beginning with the recognition by Leeuwenhoek that the 'fat globules' seen by Malpighi in the blood vessels were '*globules rouges*' (red cells), and that they were responsible for the colour of the blood.

We rapidly move forward in time, and the white cells and platelets are discovered. The principles of the phase-contrast microscope and the electron microscope are then explained in a simple way and we are introduced to the fresh flood of wonders they released. The normal process that ends in the launching of red cells into the blood stream is described (it is estimated that 250 thousand million are launched a day) together with abnormalities of the process—and so for white cells and for platelets.

Chapters deal with the coagulation of the blood, the blood groups, blood transfusion, and the fascinating story of haemolytic disease of the newborn in man and in the mules of Poitou.

The book is splendidly illustrated with electron and phase-contrast microphotographs. It cannot fail to interest both the specialist and the general reader who is curious to know about the marvellous fluid.

R. R. RACE

AUTONOMIC NERVOUS SYSTEM

British Medical Bulletin, *Vol. XIII, No.* 3. Autonomic Nervous System, *edited by W. S. Feldberg. Pp.* 153–230. *The British Council, London.* 1957. 20*s. net.*

There would be general agreement that Britain held for many years the leadership in research on the physiology

and pharmacology of the autonomic nervous system, and the fine assemblage of contributions in this number bears witness to the continuing vigour of British research in these fields. The reviewer has found all the contributions a pleasure to read, and most contained original and stimulating ideas. There were a very few statements which appeared questionable. However, it is impossible to enter into detailed criticism in this brief appraisal. Appropriately the symposium opens with two very good accounts of the metabolic processes concerned in the production of the autonomic transmitter substances —surely the best in the literature. There are also several good accounts of the various actions of chemical transmitters released by the cholinergic and adrenergic postganglionic nerve terminals. The symposium is further broadened by including critical contributions on various exceptional types of autonomic action and on anatomical and regenerative findings of particular interest to the clinical neurologist. Thus the scope of the symposium is so extensive that some aspects of it will be of direct interest to almost all medical scientists and comparative physiologists. One cannot bring this account to a close more appropriately than to quote from the final sentence of Dale's introduction, that this symposium 'gives the reader an exhilarating sense of the imminence of further developments, needing only special skill, allied with scientific vision and bold enterprise, to open new and ever widening vistas of knowledge, concerning junctional transmission in the nervous system as a whole'. J. C. ECCLES

DANGEROUS INDUSTRIAL MATERIALS

Dangerous Properties of Industrial Materials, *by N. Irving Sax. Pp. v + 1467. Reinhold Publishing Corporation, New York; Chapman and Hall Ltd, London.* 1957. *£9 net.*

This book is a necessary revision of the author's earlier work, 'Handbook of Dangerous Materials'. It has been increased in size and now deals with more than 8500 materials. In addition, the presentation of the data has been rationalized. An attempt has been made to organize the vast mass of information by correlating the properties of the materials listed in Section 10 (general chemicals) with nine previous sections, each dealing with an aspect of prevention and control of hazards which may arise in the use of these materials.

These nine sections are the outstanding feature of the book. Because of the extent of the field covered the information in them is presented in a very concise form, and is mainly confined to elementary principles. These principles are sound and clearly stated. However, there are a number of instances of overlapping of subject matter from one section to another. For this reason, and because of the amount of information in them, these sections would be of much greater value if they were indexed.

The data included and their presentation are such that the book is a useful, immediate reference work dealing with the safety of all who are involved in the use of hazardous materials. That is its objective. W. M. DIGGLE

SCIENCE OF COSMETICS

Cosmetics—Science and Technology, *edited by Edward Sagarin. Pp. xix + 1433. Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. *$25 net.*

This work is correctly described by its editors as 'the first collective volume in the history of cosmetic literature'. Its contribution to the science of cosmetics, rather than to its art, is considerable and has been accomplished by the laborious task of bringing together fifty-three sections, each with full references, and contributed by specialists in the restricted fields concerned. Each class of product is covered, from hormones through toothpastes to depilatories, both in purpose and examples of formulation, while other sections give excellent appreciations of manufacturing and physiological aspects. Modern proprietary raw materials are well covered and are identified, together with the manufacturer, by index: British readers will regret the necessity of limiting these materials largely to those of American origin.

As a general reference volume the work is a *sine qua non* to those actively engaged in the industry and provides an essential addition to the classics of de Navarre and Harry. Without detracting from its value, however, some criticism must be made. Firstly, the space given to some aspects seems misjudged in relation to their practical importance, e.g. one page on epidermal water content and permeability rates with no mention of Matoltsy's work seems hardly in correct proportion to six pages on finger-nail elongators. Secondly, a bias by contributors towards personal theories and work has not, in all cases, led to a balanced appreciation; thus the so-called HLB emulsifier selection system is undoubtedly of value, but is not the work of Schulmann also? Thirdly, the work is not free from printing errors. These are, in most cases, only annoying, but in one instance a paragraph has been rendered incomprehensible. At the price, one hopes for perfection.

R. F. L. THOMAS

EARLY HISTORY OF VOLUMETRIC ANALYSIS

The Development of Titrimetric Analysis till 1806, *by E. Rancke Madsen. Pp. 239. G.E.C. Gad, Copenhagen.* 1958. *D. Kr. 20 net.*

Dr Rancke Madsen writes of the early history of titrimetry, which he defines as the determination of a substance by its ability to participate in a chemical process. He begins with the work of Boyle on the effect of acids and alkalis on vegetable colours, which, he maintains, adumbrates the titrimetric principle, and he ends with the work of Descroizilles, who is regarded by many as the father of volumetric analysis and who invented the first practical burette. In spite of de Koninck's comprehensive survey, published in French in 1901, it is a pity that the author chose not to extend his period to 1853, when modern volumetric analysis really began with the appearance of Mohr's burette; many interesting, and some important, papers were published in the interval.

In a short review it is impossible to do justice to the author's detailed investigation of the growth of the titrimetric idea and of the changing chemical atmosphere in which it evolved, but tribute must be paid to his careful examination of more than 300 books and papers.

The book, which is a translation, would be easier to read if it had been submitted to an English editor and if the lengthy quotations in antiquated French and German had been rendered into English. Furthermore, the author's attempt to rationalize the nomenclature of analytical chemistry leads him to coin terms that will not be to everybody's taste.

Nevertheless, students of early analytical methods will be indebted to Dr Rancke Madsen for making his knowledge available. C. R. N. STROUTS

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Short notices of books

(These notices are descriptive rather than critical and are designed to give a general indication of the nature and scope of the books.)

Yeasts, *edited by W. Roman. Pp.* 246. *Dr W. Junk, The Hague.* 1957. *Fl.* 25 *net.*

This is Volume 1 of a series 'Biologia et Industria' designed to link the industries using biological raw materials with the work of biologists, chemists, and physicists. It covers the applications of the yeasts to baking, brewing, and the manufacture of wine and saké. The saké and similar yeasts have not been previously described in a European language, and the book also gives the first published account of the industrial production of food yeast written by its inventor. There are a large number of references to the original literature.

The Year Book of the Royal Society of London. *Pp.* 367. *The Royal Society, London.* 1958. 21*s. net.*

This book contains a list of Fellows and Foreign Members of the Royal Society with their current appointments and addresses, the membership of Royal Society Committees and committees formed jointly with other bodies, and the membership of the National Committees for various sciences of the International Council of Scientific Unions. It also includes a list of Royal Society research appointments, and the Statutes, Annual Report of Council, and the Funds and Accounts. Reports of the work undertaken by Professors, Fellows, and Students are also given.

Biographical Memoirs of Fellows of the Royal Society. *Vol.* 3. *Pp.* 328. *The Royal Society, London.* 1957. 30*s. net.*

There are accounts of the lives and work of 21 lately deceased Fellows in this volume, which is the third of a new series, in continuation of the Obituary Notices of Fellows of the Royal Society. The biographies are each prefaced with a photograph and contain bibliographies where relevant.

Handbook of Chemistry and Physics, (39th edition), *edited by C. D. Hodgman, R. C. Weast, and S. M. Selby. Pp. xxii* + 3213. *Chemical Rubber Publishing Co., Cleveland, Ohio.* 1957. £5 *net.*

This is a ready-reference book of chemical and physical data and is intended for the use of the practical scientist. There are many tables of mathematical functions and of a very wide variety of physical constants of substances, including solubilities, heats of formation, X-ray crystallographic data, emission spectra, densities, half-wave potentials (for polarographic analysis), and electron work functions, among very many others.

Physical Chemistry (*third edition*), *by W. J. Moore. Pp. xii* + 641. *Longmans, Green & Co. Ltd, London.* 1957. 30*s. net.*

This is an advanced textbook, written for the American student, but aimed in Britain at those reading for an honours degree in chemistry. It presupposes a knowledge of calculus, elementary physics, and first-year chemistry, and is intended to be used as the sole textbook of theoretical physical chemistry. The subjects covered include kinetic theory, nuclear chemistry and physics, structure of molecules, chemical statistics and photochemistry and radiation chemistry, as well as the normal elementary contents of physical chemistry textbooks, and the book aims at a decidedly advanced treatment. Each chapter ends with problems, answers to the problems (the main difference from the second edition), and a bibliography for further reading.

Solvent Extraction in Analytical Chemistry, *by G. H. Morrison and H. Freiser. Pp. xi* + 269. *John Wiley & Sons Inc., New York; Chapman and Hall Ltd, London.* 1957. 54*s. net.*

Solvent extraction methods are methods devised in order to give faster analyses, applicable to trace concentrations and practicable for highly complex mixtures, and this book covers both the theory and practice of the technique. The section concerned with practice deals with apparatus and techniques, a logically arranged survey of the many individual extractions and a selection of procedures for the extraction of elements. There are many references to the original literature.

Laboratory Manual of Batch Distillation, *by F. J. Zuiderweg. Pp. viii* + 126. *Interscience Publishers Inc., New York; Interscience Publishers Ltd, London.* 1957. $3.50 *net.*

The main aim of this book, which is a revision of one by Ir. J. Verheus, is to give help and guidance to the chemist who uses laboratory distillation but does not intend to specialize in the field. The theoretical principles directly bearing on the practice are explained, and the design, construction and testing of equipment are discussed. The more refined aspects, such as continuous distillation and azeotropic distillation, are omitted, but the book contains a survey of the literature which includes references to such matters. Nomographs and diagrams are given in an appendix as well as others in the text.

Le Sostanze Minerali delle Piante in rapporto all'Agricoltura e alla Zootecnia, *by Francesco Scurti. Pp. vii* + 281. *Loescher, Turin.* 1957. *Lire* 4000 *net.*

The chemical elements occurring in plants are subdivided into the macroelements such as nitrogen, phosphorus, potassium, etc., which are present in large quantities, and the microelements, boron, copper, zinc, etc., of which only minute amounts are found, separate chapters being devoted to each element. Their importance in plant physiology and soil behaviour are considered and, where appropriate, their excess or deficiency is discussed in relation to plant diseases. Each chapter is provided with a full bibliography drawn from international sources.

General Pathology (*second edition*), *edited by Sir Howard Florey. Pp. xv* + 932. *Lloyd-Luke (Medical Books) Ltd, London.* 1958. 84*s. net.*

This book is based on lectures delivered at Oxford to students who have read the Honours School of Physiology. It does not attempt to survey general pathology completely, though some chapters have been added in this edition towards this end, but to deal with subjects in which one or other of the authors has a special interest.

There are 43 chapters treating some of the fundamental changes which take place in the body in response to injury, using this word in a broad sense, and the book is intended to arouse the better student's interest in the functional as well as the morphological aspects of pathology.

Notes on contributors

J. E. ALLEN,
B.Eng., Ph.D., A.M.I.E.E.,

Was born in 1928 and educated at the Alsop High School, Liverpool, and at the University of Liverpool. From 1949 to 1952 he studied the pinch effect in high-current spark channels, under the direction of J. D. Craggs, in the Electrical Engineering Department, University of Liverpool. Since 1952 he has worked in the Controlled Thermonuclear Reaction Group at the Atomic Energy Research Establishment, Harwell, and has carried out experimental and theoretical work on various aspects of plasma physics.

HERBERT DINGLE,
D.Sc., A.R.C.S.,

Was born at Brixton, London, in 1890, but spent his early life at Plymouth, where, after leaving school, he was for some years employed by the local education authority. In 1915 he entered the Royal College of Science, London, as a student of physics, and after graduating remained at the college as Lecturer, Assistant Professor, and Professor of Natural Philosophy. In 1946 he was appointed to the Chair of History and Philosophy of Science at University College, London, from which he retired as Emeritus Professor in 1955. He is a member of many British and foreign learned societies, and has been secretary and president of the Royal Astronomical Society. In 1936 he was Lowell Lecturer at Boston, U.S.A., and in 1927 and 1932 was a member of the British Government Eclipse Expeditions (the 1940 expedition, of which he was also a member, was cancelled). His scientific work has been chiefly concerned with spectroscopy (especially in its applications to astronomy and medical and industrial problems), relativity, and the philosophical aspects of science.

G. E. R. DEACON,
C.B.E., D.Sc., F.R.S.E., F.R.S.,

Was born in 1906 and was educated at King's College, London. He worked with the Discovery Investigations from 1927 to 1939, making four voyages to the Antarctic and publishing a number of papers on the oceanography of the Southern Ocean. After four years war service in anti-submarine research, he directed the oceanographic research group in the Admiralty Research Laboratory, Teddington, and has been Director of the National Institute of Oceanography since it was founded in 1949.

SIR HAROLD SPENCER JONES,
K.B.E., M.A., Sc.D., D.Phil., Ll.D., F.R.S., F.R.S.E.,

Held the position of Astronomer Royal from 1933 to 1955. He was born in London in 1890 and was educated at Latymer Upper School, Hammersmith, and Jesus College, Cambridge. From 1923 to 1933 he held the post of His Majesty's Astronomer at the Cape of Good Hope. He was knighted in 1943. He has received the Royal Medal of the Royal Society, the Gold Medal of the Royal Astronomical Society, the Janssen Medal of the Astronomical Society of France, the Bruce Gold Medal of the Astronomical Society of the Pacific, and the Gold Medal of the British Horological Institute. He is an Honorary Fellow of Jesus College, Cambridge, a past president of the Royal Astronomical Society, of the British Astronomical Association, of the Institute of Navigation, of the International Astronomical Union, and of the Society for Visiting Scientists. He is president of the British Horological Institute. He is a foreign member of many national academies and an honorary member of many Dominion and foreign astronomical societies.

GEORGE SALT,
Sc.D., F.R.S.,

Was born at Loughborough in 1903 and educated at C.H.C.I., Calgary, Alberta, The University of Alberta, Harvard University, and the University of Cambridge. Is now Lecturer in Zoology in the University of Cambridge, and a Fellow of King's College, where he was formerly also Tutor for Advanced Students. His principal research has been on insect parasites, but during the war he worked on wireworms and other insects in soil. He has engaged in the control of insect pests of various crops in several parts of the world and much of his academic research on insect parasites has been done with an eye to the biological control of pests. He is an editor of the Cambridge Monographs in Experimental Biology, and has published numerous papers on ecology, entomology, and parasitology.

C. B. AMPHLETT,
B.Sc., Ph.D.,

Was born in 1920 and educated at the University of Birmingham. During the war he carried out research at Birmingham on the chemistry of uranium for the Tube Alloys atomic energy project, and then joined the joint British-Canadian atomic energy team in 1944, at first in Montreal and later at Chalk River. He returned to England in 1947 to join the Chemistry Division of A.E.R.E., Harwell, and worked until 1952 on the chemical effects of ionizing radiation. Since 1954 he has been engaged on studies of inorganic ion-exchange systems with reference to their possible applications in the atomic energy field.

J. B. THOMAS,
Ph.D.,

Was born in Java in 1907 and studied plant physiology at the State University of Utrecht. In 1940 he went to the then Royal Botanical Gardens at Buitenzorg, Java, to study the water economy of tropical plants. He was a prisoner of war during the Japanese occupation of Java. He returned to the Netherlands after the war and in 1947 joined the Biophysical Research Group at Utrecht where he was appointed a lecturer in biophysics in 1957. His main research interest is in photosynthesis.